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SCIENCE

25 July 1958

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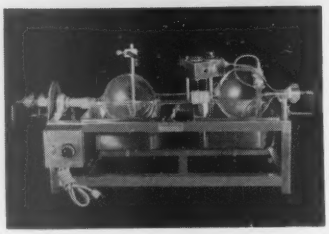

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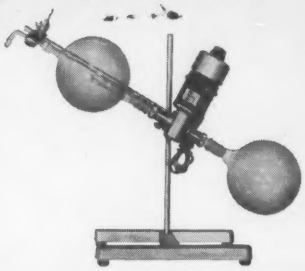

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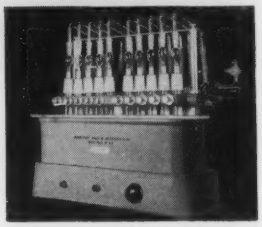

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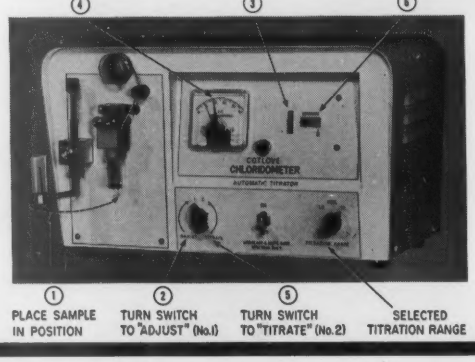

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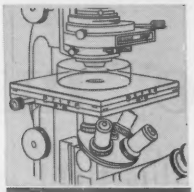

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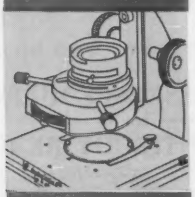

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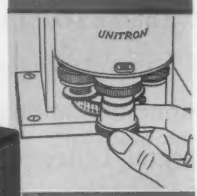

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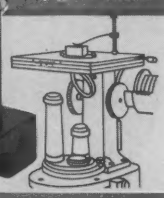

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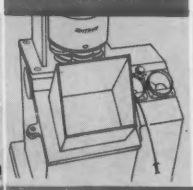

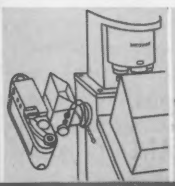

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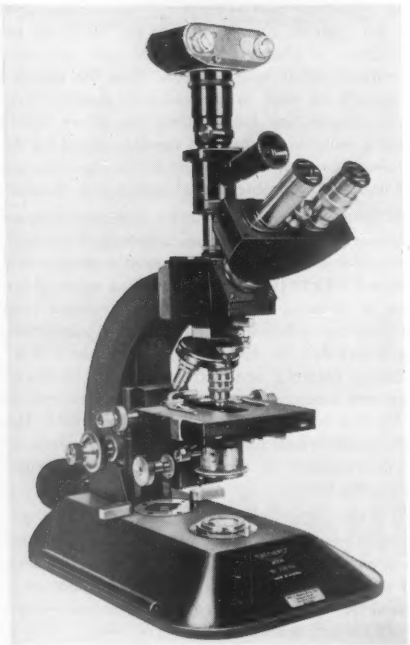

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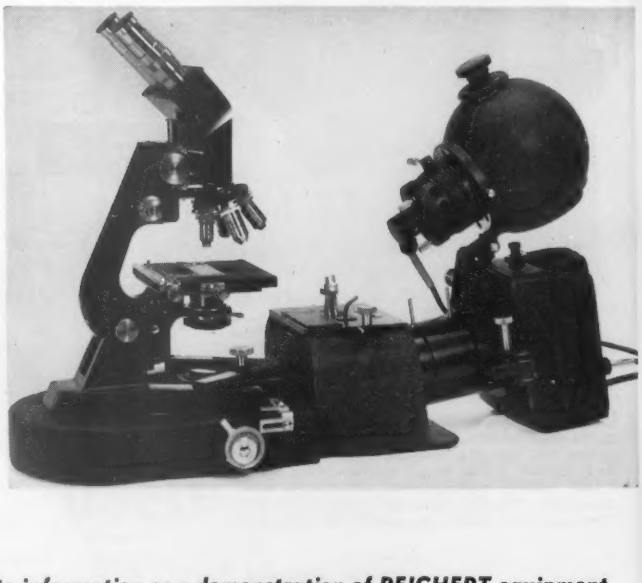

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USSR and *Amerika*

One aspect of the cultural exchange between the Soviet Union and the United States that has attracted little notice is the production of monthly magazines by each country for distribution in the other. The magazines, *USSR* and *Amerika*, are both richly illustrated slick-paper magazines of generous and attractive format. What does each country think it important to tell the other? How are the magazines alike? How do they differ?

The magazines resemble each other in that they both devote much space to articles about medical care, schools and teachers, art museums, artists, the ballet, symphony orchestras, the theatre, sports, fashions, family life, vacations, and vacation resorts. Judging by the pictures, the people in both countries are healthy and happy, at work and at play.

But in emphasis and attitudes in some fields the magazines differ sharply. *USSR* is obviously written for the American audience; unfortunately, so is much of *Amerika*. Aside from two articles about the production of *War and Peace* as a movie and as a television show, none of the articles in *Amerika* is specifically directed to a Russian audience, although special articles about the fall of parity and the functions of Congress and of the Supreme Court are surely informative to that audience.

For the most part, *Amerika* depends upon reprinted material—that is, material originally written for an American audience and published in books or such magazines as *Life, Look, McCall's, The Lamp, Friends*, and so on. Of the 125 articles in the last seven issues of *Amerika*, 77, or more than 60 percent, are reprints. Many of these are of good quality and present an adequate picture of some aspect of American life. What they lack for the present purpose is the background that most Americans—but few Russians—would be expected to know. The Russians on the other hand take pains to sketch in the background; they take nothing for granted. The Russians describe their air transport network and their large, fast, new planes; the Americans reprint an article about America's little airlines, some of which are still flying DC-3's. *USSR* describes how deserts are made to bloom under irrigation; how hydroelectric power is increasing and how it is fed into a national grid; and how tractors and other mechanical aids help the farmer. *Amerika* has little that is comparable in scope.

The remedy is clear. *Amerika* should become less dependent on reprints and should commission articles that give comprehensive pictures of some of our accomplishments. We suggest articles on the Grand Coulee Dam, the TVA, wheat farming, agricultural research, the oil industry, automation and the machine tool industry, great industrial cities, the U.S. contribution to antibiotics, nuclear reactors, research accomplishments in optical and radio astronomy, the invention of the transistor, synthetic diamonds, America and the IGY, biographical sketches of noted American scientists, the National Science Foundation and basic research, foreign students in the United States, and the American Pavilion at Brussels.

The United States has a great and exciting story to tell; it is up to *Amerika* to tell it.—G.DuS.

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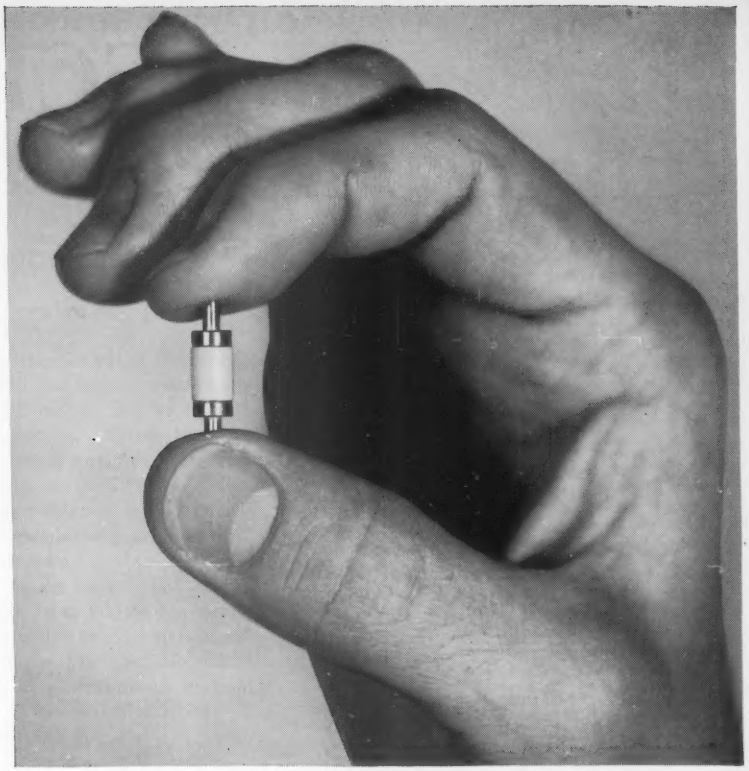

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How the radio art can be improved through solid state science is illustrated by a recent development at Bell Telephone Laboratories. To make voice signals travel by microwaves they must first be "converted"—caused to vibrate at billions of cycles per second. To date, it has been possible to accomplish this conversion only at the cost of appreciable loss of signal energy. Could a more efficient converter be provided?

In the field of solid state science it was known—as a laboratory curiosity—that semiconductor diodes can be made not only to convert the frequency of signals, but also to amplify them. At Bell Laboratories Dr. Arthur Uhlir, Jr., and his associates calculated that this amplifying action could be put to practical use. They proved the point by developing a junction diode converter which can deliver up to 40 times as much signal energy as previous converters.

This efficient new converter will be applied in a new Bell System microwave highway able to transmit thousands of telephone conversations and a dozen television programs simultaneously at six billion cycles per second. In other forms it is being developed, under Signal Corps contract, for radar and military communications where more efficient frequency conversion can also be used to advantage.

This development is an example of the many different ways in which Bell Laboratories works to improve your telephone service and communications at large.

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25 July 1958, Volume 128, Number 3317 SCIENCE

CURRENT PROBLEMS IN RESEARCH

New Dimensions in Seismology

Earthquakes are characterized by geographical position, instant of occurrence, depth, and magnitude.

C. F. Richter

The occurrence of an earthquake is a good example of a point event in space-time. In the 1850's, when the foundations of seismology were laid, an earthquake was held to originate in a sudden and probably explosive change affecting a subterranean volume small enough to be taken as a point, for most purposes. This point was termed the *hypocenter*; the spot vertically above it on the surface of the earth was termed the *epicenter*.

On this basis, there are four quantities to be determined; two give the geographical position of the epicenter, a third is the depth of the hypocenter, and the fourth is the instant of occurrence. Various means were tried to eliminate the last two and concentrate on locating the epicenter.

When it became known that such earthquakes as took place in Japan in 1891, and in California in 1906, were accompanied by fracturing of the surface rocks along lines extending for hundreds of miles, it was thought that the ideas of hypocenter and epicenter would have to be abandoned. Instead, the newly developed methods of instrumental seismology furnished revised definitions. They also compelled seismologists to consider the time dimension, for the new definition of the hypocenter, as we shall see, was intimately connected with the idea of a specific origin time.

At first, progress was assisted by the fact that most earthquakes originate at relatively shallow depths, of the order of 15 to 30 kilometers. After 1922, discovery of earthquakes at depths as great as 700 kilometers made it necessary to work with all three spatial dimensions.

A still later development assigns to each earthquake a numerical measure called its magnitude; here, in effect, we are dealing with a fifth dimension.

Duality

An earthquake is a dual event, and consequently seismology is a dual science. On the one hand, an earthquake is a sudden subterranean occurrence. In most earthquakes, including all large ones, this occurrence is not an explosion but a shearing fracture, which may extend to the surface, there to present itself for inspection and study by field geologists. On the other hand, sudden fracturing sends out through the solid material of the earth two principal types of elastic waves, with velocities roughly in the ratio 1.7 to 1. Seismologists designate the faster waves *P* (from *primae*) and the slower, *S* (from *secundae*).

Except for their generally low frequency of oscillation (long period, great wavelength), the *P* waves are physically identical with ordinary sound waves. They are waves of alternating compression and dilatation, with particle displacements longitudinal, along a ray. Waves of the *S* type exist only in solids; they represent shearing deformation, with particle displacements transverse, at right angles to the ray. *S* waves consequently are capable of polarization.

Near the epicenter of a large earthquake these waves arrive at the surface with sufficient energy to occasion perceptible and even violent vibration. Shaking due to these elastic vibrations is the immediate cause of most earthquake damage and of many other common effects of earthquakes.

Instruments which write a continuous magnified record of such earth motion are seismographs. The first effective ones were built about 1880, but some years passed before it was realized that seismographs might be capable of recording earthquakes originating at great distances from the point of observation—teleseisms, as they are now termed. The first identified teleseismic record was written on 17 April 1889, when an instrument set up by von Rebeur-Paschwitz at Potsdam registered a disturbance due to an earthquake in Japan. This date is a milestone, as important in the history of seismology as the night when Galileo turned his first telescope on Jupiter is important in the history of astronomy.

With the discovery of teleseismology, the study of earthquakes was divided into two cooperative disciplines. Instrumental recording, and the interpretation of the resulting seismograms, called for all the normal techniques of laboratory physics, while interpretation of the phenomena of great earthquakes continued to challenge the skill of the field geologist. Teleseismology and field seismology have advanced together.

This article is concerned with a few recent developments only, and with instrumental seismology almost exclusively; at least as much space would be needed to cover the results of field work.

"Instrumental Epicenter"

Earthquakes first studied were those for which seismologists could locate an epicenter from field observations—using the center of the most violent shaken area or the vicinity of surface fractures. Epicentral distance was usually given as the angle Δ at the center of the earth

The author is professor of seismology at California Institute of Technology and a member of the staff of the Seismological Laboratory, Pasadena, Calif.

between the radii to the epicenter and to the recording station. It developed that the times of arrival of a given type of elastic waves at the stations for any one earthquake depended primarily on Δ:

$$t = t_0 + f(\Delta)$$

where t_0 is an instant taken as that of occurrence of the earthquake, and $f(\Delta)$ is a universal function, the same for all epicenters and stations, no matter where they are located on the earth's surface.

This surprising result was confirmed with progressively greater accuracy as the sensitivity of seismographs and the precision of time-keeping improved. For normal large earthquakes, the recorded times at hundreds of stations, distributed all over the world, can be fitted to a standard time-distance table or plotted curve. The individual deviations from the standard are not usually over 2 seconds, although even for the fastest waves the elapsed times, represented by $f(\Delta)$, range up to more than 20 minutes. This, of course, depends on making the right selection of the epicenter from which the distances Δ are measured; various graphical and analytical methods were developed for the purpose and applied systematically in a huge synoptic publication, the *International Seismological Summary*.

The fact that it is possible to find such a satisfactory, instrumentally determined epicenter has two important implications. First, it shows that the earth has a high degree of spherical symmetry. Indeed, it is possible to detect and compensate for the slight flattening of the terrestrial spheroid, which amounts to only 1 part in 297. Second, there is good reason to suppose that in large earthquakes the actual process of fracturing goes on for some minutes; yet, an origin time t_0 can be fixed within 1 or 2 seconds. Further physical explanation is needed. This is found in the reasonable assumption that fracturing extends itself with a speed less than that of elastic waves through unfractured rock. This being so, the first elastic waves to arrive at a distant station must be those which originated at the point of initial rupture. The "instrumental" epicenter and hypocenter represent the position of that point; the origin time t_0 represents the instant at which rupture began.

A further factor of significance in the successful use of a single set of time-distance tables is the fact that the effect of a slight increase in depth of the hypocenter is nearly equivalent, so far as distant points are concerned, to a slightly earlier origin time. Close distinction is possible only when recorded times are available for both near and distant stations.

Highway offset horizontally by faulting, as indicated by the painted center strip, in Imperial Valley, near Meloland, California, May 1940. [Photograph by J. P. Buwalda. From *Elementary Seismology*]

Deep Earthquakes

In 1922, H. H. Turner published a discovery resulting from his work in editing the *International Seismological Summary*. Certain earthquakes, some quite large, showed recorded times differing systematically from those of the standard time-distance tables. In particular, the times of arrival at very distant stations, in the region of the antipodes of the epicenter, were early relative to the times of arrival over short epicentral distances. These time discrepancies sometimes amounted to as much as 2 minutes, and the whole phenomenon was too systematic to be explained as due to accidental errors. Moreover, careful computation showed that these data were in good agreement with the expected findings for earthquakes with hypocentral depths of as much as 600 or even 700 kilometers.

This result was not generally accepted at first and began to receive serious attention only after K. Wadati, in Tokyo, published his results, derived independently from seismograms written at numerous stations in Japan, confirming the occurrence of deep earthquakes in that region. Beginning in 1931, presentation of a series of papers by many investigators settled the question, and standard time-distance tables were set up for all hypocentral depths (h) down to 700 kilometers. We thus have:

$$t = t_0 + f(\Delta, h)$$

Meanwhile, observations for a large majority of recorded earthquakes continued to fit the established data for shallow or "normal" depth.

The exploration of this new dimensionality raised many new questions bearing on the nature and mechanism of earthquakes. Because of the great pressures at depths of several hundred kilometers, theory at first tended to regard deep earthquakes as due to some explosive process rather than to shearing fracture, which is generally supposed to be the cause of shallow earthquakes. This theory, however, is almost completely ruled out by the character of the recorded seismograms, which show a form of displacement which can originate only in shearing fracture. A further fascinating question, to which at present there is no completely satisfactory answer, is that of why earthquakes occur (in certain regions only) down to depths of about 700 kilometers and no deeper. The problem is sharpened by the fact that, although frequency of occurrence generally decreases with increasing depth, there is a fairly abrupt pinch-

ing off at the lowest levels, which vary from about 600 to about 700 kilometers in depth, according to the region. Some of the deepest known shocks were major earthquakes.

Fifth Dimension

In 1931 the newly established seismological laboratory at Pasadena was preparing to issue regular bulletins on earthquakes in southern California. Since the list would include 200 or more earthquakes a year, it was felt that some indication was required to distinguish large from small disturbances; otherwise misinterpretation might follow, and undue alarm be raised. It was hardly practicable to characterize these earthquakes on the basis of perceptibility to persons or of other gross effects. Many of the epicenters were in almost totally unpopulated mountain or desert areas, and some were off the coast. Often seismographs at several stations wrote large records of an earthquake which otherwise would have passed unnoticed.

The way out was found in a direction already taken by K. Wadati, whose work with Japanese deep shocks has been cited. For several earthquakes he plotted the recorded amplitude of ground motion against the corresponding epicentral distance Δ of the recording station. The resulting curves were crudely parallel; the larger the earthquake, the higher the curve appeared on the plot.

A similar attempt was made at Pasadena, but the range in recorded amplitudes was awkwardly large. To get a more manageable plot, the logarithms of the amplitudes were used. This improved the parallelism of the several plotted curves and suggested the drawing of a representative standard curve, parallel in general to any individual curve, which therefore would differ in level by a constant. This constant, characteristic of the individual shock, was termed its *magnitude,* lettered M, and defined in effect by

$$\log A\,(\Delta) - \log A_0\,(\Delta) = M$$

Here A_0 as a function of Δ represents the standard shock, whose amplitudes accordingly correspond to magnitude zero. A shock smaller than the standard would have a negative magnitude; to obviate this, the zero shock was arbitrarily chosen as very small, corresponding to that from an earthquake at the lowest level, recorded with high magnification at very short distances.

For convenience the work was done, not with the true amplitudes of earth displacement, but with the recorded trace amplitudes read directly in millimeters on seismograms of the Anderson-Wood torsion instruments, then in operation at all the stations in southern California. These instruments were all designed to have the same operating parameters: free period of the pendulum, 0.8 seconds; static magnification, 2800; damping, slightly less than critical. The dynamic magnification should therefore be the same for waves of the same period and should not greatly affect the determination of M. To fix the zero level, a shock recorded on the torsion seismogram with amplitude of 1 millimeter at $\Delta = 100$ kilometers was defined as having magnitude 3.

The whole procedure involves very crude assumptions, partially contrary to observation. In effect, it is supposed that two seismograms recorded at the same distance can be derived one from the other by merely multiplying all displacements by a constant. This is not so. Experience shows that the frequency distribution varies with magnitude; large and small earthquakes have appreciably different elastic-wave spectra.

The name *magnitude* was purposely chosen because of analogous use of the term in astronomy. The scale of stellar magnitudes is also logarithmic, though less simply so. In a sense, the astronomical scale is reversed: the brighter the star, the smaller its magnitude. Other familiar logarithmic scales are the decibel and the *p*H scale.

Since only approximate and local use of the magnitude scale was expected, there was little concern about the roughness of the assumptions. There was some astonishment when it proved possible to assign magnitudes to the nearest half unit over the range from 0 to at least 6; in other words, instead of crude separation into large and small, twelve definite levels of magnitude could be distinguished. Later work has extended observed magnitudes above 8 and has made it reasonable to cite them to the tenth of a unit. Magnitude thus becomes truly quantitative and establishes itself as the fifth seismological dimension.

Magnitudes for Distant Earthquakes

Success of the magnitude scale in California does not guarantee its applicability to other areas. The variation of amplitude with distance can be represented by a single function only if the hypocentral depth and the constitution and structure of the upper part of the earth's crust are nearly constant. In principle, there would be no problem in extending the scale to earthquakes in California as recorded at great distances, but those sufficiently large to write decipherable seismograms in Europe, say, are relatively rare. Moreover, the original definition ties the scale to the perform-

Damage due to shaking, near Meloland, California, May 1940. [Photograph by J. P. Buwalda. From *Elementary Seismology*]

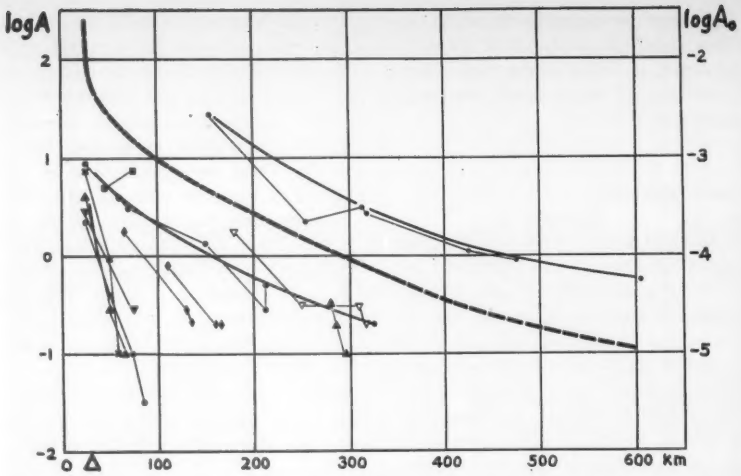
ance of a particular type of instrument, differing (especially in its short pendulum period) from most of those in use abroad.

Accordingly, the principle of the magnitude scale was applied to different material. Seismographs of the types most in use record the maximum of a normal shallow teleseism as a train of fairly regular waves with period near 20 seconds. These are surface waves, guided along the surface of the earth, with prevailing period determined by the average crustal structure, which acts selectively to transmit frequencies in the range near 3 cycles per minute. Many stations report the recorded maximum amplitude of earth motion in these waves, in microns, as calculated from the known parameters of the seismographs used. Accordingly, it was possible to plot the logarithms of these maxima against distance over the earth's surface and to define the magnitude in terms of a mean curve. Moreover, it is possible to calculate the expected curve theoretically; it should approximate closely the simple type

$$\log A = a + b \log \Delta$$

This proves to fit the observations well at many stations, provided that a proper choice is made for the constant characteristic of the individual station (which is an expression of the effect of local ground conditions).

The zero for M could be selected arbitrarily; it was originally set by using various means of correlation with the California local scale, with the result that a shock of magnitude $M = 7.0$ records with a maximum amplitude of 100 microns at a distance of 80° of a great circle.



Origin of the magnitude scale. Data for earthquakes in southern California, January 1932. The dashed curve represents adapted data for a typical shock. [From *Elementary Seismology*]

Magnitudes for Deep Earthquakes

Theoretically, the surface waves generated by a given earthquake should decrease exponentially with increasing hypocentral depth. Observation shows that deep earthquakes record with very small surface waves, even when the rest of the seismogram, due to elastic waves through the body of the earth, is large. Accordingly, any magnitude scale for deep shocks must be based on the amplitudes of body waves. Development of a scale of this type was accomplished by Gutenberg; he first set up such a scale for shallow earthquakes whose magnitude M had been determined from surface waves and then adjusted it to deep shocks. Because of the wide range in amplitude and period within the body-wave groups, it developed that meaningful results could be obtained only by working, not with the recorded ground amplitude A, but with the quotient of A and the period T, modifying the definition of magnitude so that

$$\log (A/T) - \log (A_0/T_0) = m$$

where m is a new variety of magnitude. The quotient A/T is a measure of the velocity of the vibrating particle within the wave and is consequently related to its kinetic energy; this is even more satisfactory physically than dealing with the amplitude alone.

For shallow shocks, the quotient A_0/T_0 was chosen to make m coincide with M near the value 7, in the middle of the best observed range of magnitudes. For deep shocks, the effect of varying depth on the amplitudes of body waves was calculated theoretically; the uncertainties of this calculation were on the whole less than those generally associated with the determination of magnitude. The only critical assumption here was that the transition is continuous; if there is any level at which there is a sudden absorption or other loss of energy, then correlation between shocks originating above and below that level is impaired. Shebalin has presented evidence for such absorption at a depth of about 60 kilometers; if substantiated, this would imply that magnitudes of deep shocks have been somewhat overestimated relative to those of shallow ones.

The magnitudes m proved internally consistent and could be assigned with as much assurance as the magnitudes M from surface waves. However, the two scales diverge; between m and M, as determined for teleseisms, a linear relation holds with considerable accuracy:

$$M = 1.59m - 3.97, \; m = 2.5 + 0.63M$$

This gives $m = M$ near 6¾.

The two magnitude scales are thus related much as the two familiar scales of temperature are related; any choice between them is obviously a matter of convenience and of experience in use. Initially Gutenberg invariably reduced m to M, and results were published in that form. However, during work on a collaborative revision of magnitudes and of the magnitude-energy relation, undertaken with me, he abruptly reversed his position. The scale m has certain advantages; in theory, it is more simply related to earthquake energy than the scale M. Accordingly, in joint publication it was suggested that m be made the basis where energy calculations are involved—or, still better, that results be stated directly in terms of energy in ergs. To reduce the use of exponents, Shebalin has suggested employing a quantity E', which is the logarithm of the calculated energy when expressed in megajoules (units of 10^{13} ergs).

Since much work had been done and since many workers continued to determine and report magnitudes in terms of the M scale, there was objection to what appears to be an unnecessary change. With this I must agree, for the present at least. There is nothing wrong with either the m or the M scale for teleseisms; both are solidly established, and

their mutual relation is well determined. However, neither can now be related definitively to the values of magnitude as determined by the original method for California local shocks. Such magnitudes necessarily are still determined and reported on the old basis. Until such time (probably not more than a few years in the future) as the local scale can either be replaced by, or used to adjust, the teleseism scales, I feel that any general use of the m scale can only lead to errors, and make it more difficult to deal with the objections raised to the magnitude scale as such, which usually are based on misinformation.

Kárník at Prague has lately made an earnest effort to base a scale for earthquakes at short distances on the quotient A/T, thus making such a scale continuous with the m scale. The test of this method will require further observations in the future, in order to establish that it does not constitute a fourth new scale, divergent from the other three.

Applications of the Magnitude Scale

It is often taken for granted that the only purpose of the magnitude scale is the calculation of earthquake energy, and this leads repeatedly to proposals to replace it by an absolutely defined scale like Shebalin's E'. This is a misunderstanding; the original purpose, both simpler and more easily fulfilled, was to combat nonsensical errors in compiling statistics of earthquakes and in studying the geography of earthquakes and earthquake risk.

A quantitative result, independent of energy calculations, is the finding of rather regular decrease in the frequency of earthquake occurrence with increasing magnitude. If we define $N(M)$ as the number of earthquakes of magnitude M or greater per unit time (usually per year) in a specified region, or even for the whole world, then to a fair approximation

$$\log N = A - BM$$

For a large area in southern California, $A = 4.8$, $B = 0.9$; these data are derived from statistics of shocks with M ranging from 2.5 to 6. For the world, we may use $A = 8$, $B = 1$. However, earthquakes of the larger magnitudes are relatively less frequent than the simple relationship would indicate. Gutenberg has lately found results equivalent to $A = 9.1$, $B = 1.1$, for magnitudes of 7 and over. Approximate annual frequencies for the world are given in Table 1. The data for lower magnitudes are extrapolated from statistics for small areas. The total number of shocks in excess of magnitude 2.5 must be well over 150,000, and if very small shocks are counted, a million a year is a reasonable estimate of the total number.

Table 1. Approximate annual frequency of earthquakes with respect to magnitude, throughout the world.

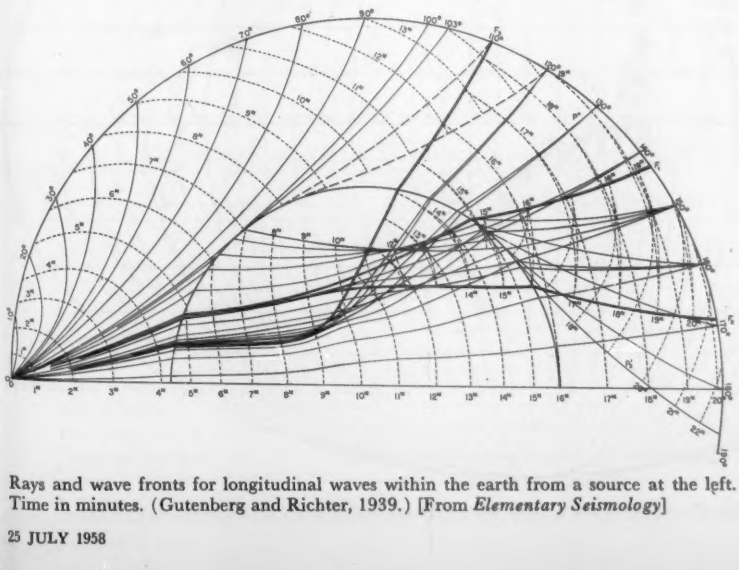
Magnitude	Number
9.0 or over	0
8.0 – 8.9	2+
7.0 – 7.9	18
6.0 – 6.9	150
5.0 – 5.9	800
4.0 – 4.9	6,200
3.0 – 3.9	49,000

In the preinstrumental period of seismology, the supposed facts about the geographical occurrence of earthquakes were distorted, to a degree then unsuspected, by the dependence of information on population. Many early maps of seismicity showed concentration in isolated spots surrounding the oldest established centers of culture. There was astonishment when the first results of world-wide instrumental recording showed a majority of large earthquakes occurring under the sea, and many in remote regions of the continents. Even data on instrumentally determined epicenters can be misleading if no attention is paid to magnitude; thus, maps showing epicenters taken from the *International Summary* gave an utterly false impression of high seismicity in Europe. The apparent concentration of earthquake locations in that region is due merely to a concentration of recording stations.

When deep-focus earthquakes were first investigated, it appeared that even in the *International Summary* some gross errors in location had resulted from mistaking deep shocks for shallow. In order to derive reasonably acceptable geographical information, it proved necessary to revise the entire corpus of located epicenters, determine magnitudes wherever possible, and construct maps accordingly.

Eighty percent of large earthquakes, and at least as large a proportion of small ones, occur in a wide belt surrounding the Pacific, with branches extending into the East and West Indies. Most of the remainder are included in another belt extending across Asia and through the Mediterranean region of Europe. Other active belts extend through the seas; an important one follows the Mid-Atlantic Ridge.

Earthquakes are classified by hypocentral depth into shallow (down to 60 kilometers below the surface), intermediate (70 to 300 kilometers), and deep (300 to 700 kilometers). No shocks are known to occur at depths greater than about 720 kilometers. Intermediate earthquakes occur in belts that follow in general the course of the shallow earthquake zones, but with deviation or displacement of 100 or 200 kilometers horizontally. They tend to follow lines of active volcanoes. Though most of them occur around the Pacific, they are common in the trans-Asiatic belt. Under the Hindu Kush in central Asia there is the most remarkably persistent earthquake source in the world; at a depth near 220 kilometers, in a volume not over 20 kilometers in diameter,



Rays and wave fronts for longitudinal waves within the earth from a source at the left. Time in minutes. (Gutenberg and Richter, 1939.) [From *Elementary Seismology*]

nearly 100 earthquakes with magnitudes from 5 to 7 have occurred in 50 years.

Until 1954, shocks at depths of more than 300 kilometers were known only from the Pacific region; but on 29 March 1954 an earthquake of magnitude 7 took place at a depth of 640 kilometers under southern Spain.

Applicability of the magnitude scale to estimation of earthquake risk is evident. Considerations of magnitude are highly important in regions, such as California and parts of the U.S.S.R., where population and industrialization are now expanding into areas previously unoccupied, some of them near known earthquake sources. Past records of shaking in the older, populated centers are here insufficient. Instrumental records, as well as geological field evidence, must be employed in estimating risk. Thus, in the California region an earthquake of magnitude between 6 and 6½ occurs on the average every year or two. Most of these have hitherto affected thinly populated districts. The few originating close to the towns have been severely damaging, like the earthquake at Santa Barbara in 1925, or locally disastrous, like the Long Beach earthquake of 1933. Every California community has to consider the possibility of such an event in its immediate area. This is the more constant and easily evaluated risk; there is also the continually impending probability of a really great earthquake, of magnitude over 8, like the "San Francisco" earthquake of 1906 (equally great shocks occurred in California in 1857 and 1872).

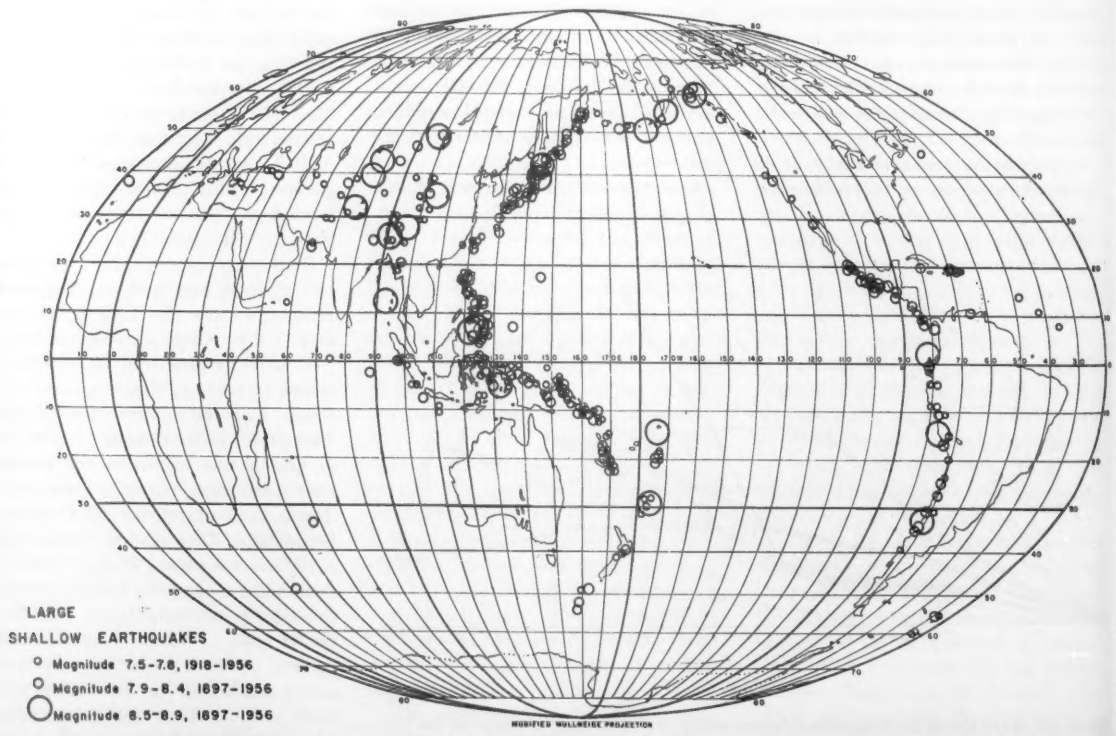
Present uncertainties in relating energy to magnitude are occasionally misapplied to make the magnitude scale appear somehow speculative. It is not speculation but cold fact that seismograms written in Europe for the 1906 earthquake were about 100 times as large as those for the 1933 Long Beach shock; it is also fact that they were over three times as large as those for the July 1952 earthquake in Kern County, while the earthquake that did damage at Bakersfield in the following month (of magnitude 5.8) was barely recorded in Europe.

Magnitude and Intensity

The magnitude scale supplements, but by no means replaces, the older intensity scales which apply to effects observed in the field. The Rossi-Forel scale of ten grades, long in use as an international standard, has now been replaced by several approximately equivalent versions of Mercalli's intensity scale, with twelve grades.

The grades of the intensity scale do not represent measurement and are not primarily defined in physical terms. They are purely descriptive and are ordinarily assigned Roman numerals to discourage treating the scale degrees as measured quantities. For example, intensity I means that the shaking was generally not perceptible to persons; II, that it was barely felt; IV, that it was strong enough to rattle windows and dishes; VII, that it was sufficient to cause general damage to weak construction; XII (the highest grade), that artificial structures were almost totally destroyed, with violence sufficient to hurl stones out of their seats and posts from their holes. The various levels are described in considerable detail.

The relationship between earthquake intensity and magnitude is one familiar in all branches of physics where radiations are observed. It is the same as that between the illumination seen on a surface and the candle power of the light source, or as that between the signal strength on a particular radio receiver and the power of the transmitting station.



Occurrence of large shallow shocks. [From *Seismicity of the Earth*, redrafted with additions for *Elementary Seismology*]

This extremely simple matter has been received with a surprising degree of misunderstanding and objection. There seems to be gross popular ignorance about the nature of an earthquake; its relation to radiation is not grasped, and there appears to be a subconscious idea on the part of many people that an earthquake shakes a whole area with equal violence. Work preliminary to formulation of the magnitude scale showed that actual ground motion drops to about 10 percent at a distance of 40 kilometers (or 25 miles) from the epicenter; the statement of this fact is often received with astonishment. Even seismologists often overlook the rapid decrease with distance and overestimate the magnitudes of large earthquakes whose epicenters are nearer to the seismological stations than is usually the case.

Engineers, accustomed to using the intensity scale, have been puzzled by the use of magnitudes. We are often asked why we use both magnitude and intensity scales, and whether use of a single scale would not be better; such a question ignores the fact that two different objectives are involved. Objections are sometimes based on the idea that magnitude represents a calculated energy; this is the reverse of the actual procedure. Complaints of "confusion" often are heard, sometimes coming from those who have been using intensity ratings as if they were magnitudes, thus really confusing their own thinking.

Magnitude and Energy

It is evident that magnitude should be at least a rough indication of the total energy radiated from the earthquake source in the form of elastic waves. Evidently this is less than the potential energy of elastic strain stored in the rock prior to the earthquake; some energy must be dissipated as heat, some must go into crushing and deformation, and some, in many earthquakes, is expended in raising crustal blocks against gravity.

If the crude assumptions implicit in the magnitude scale were valid, so that all seismograms written at a specified distance could be derived from each other by applying constant multipliers to the whole motion, then the radiated energy should vary as the square of the recorded amplitude, and we should find that

$$\log E = A + 2M$$

where E is the radiated energy and A is a constant.

Actually, the seismic spectrum changes with increasing magnitude; the larger the earthquake, the more likely it is that the largest deflection on the seismogram is due to a long-period wave carrying a disproportionately small fraction of the energy. Such a recorded wave will raise the magnitude; hence, if the energy-magnitude relation is in the general linear form

$$\log E = A + BM$$

the coefficient B should be less than 2.

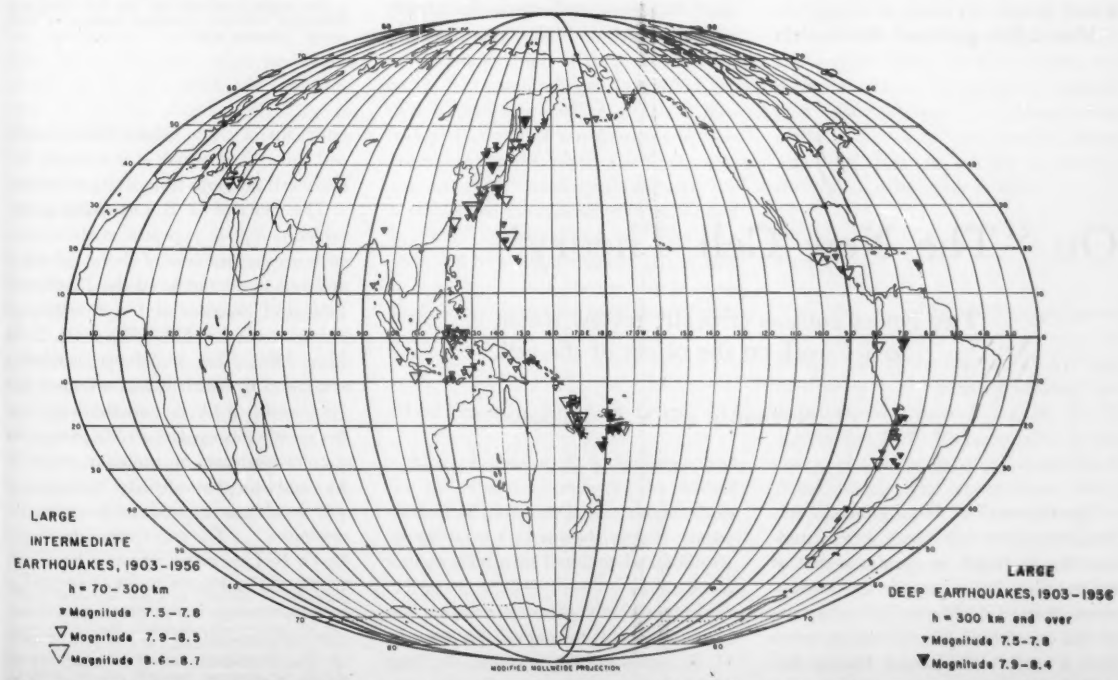
In working with teleseisms, comparison of body waves and surface waves shows that, with increasing magnitude, the surface waves of 20-second period increase relatively to the body waves, as is shown by the relation between m and M. Again the coefficient B must be less than 2. Gutenberg has set up the relation

$$\log E = 5.8 + 2.4m$$

which is equivalent to

$$\log E = 11.4 + 1.5M$$

This is an extremely rough relationship, but good enough for many approximate calculations. It shows, for example, that the energy radiated in the largest earthquakes is of the order of 10^{24} ergs. Combined with the statistical magnitude-frequency relation, it shows that nearly all the annual release of seismic energy occurs in earthquakes of magnitude 7 and over; smaller shocks are not sufficiently frequent to contribute more than an insignificant fraction.



Occurrence of large intermediate and deep shocks. [From *Seismicity of the Earth*, redrafted with additions for *Elementary Seismology*]

Magnitude, Strain Release, and Earthquake Mechanics

Hugo Benioff has applied the magnitude scale to a systematic investigation of the succession of aftershocks which follow almost every important earthquake. A great earthquake represents a readjustment of local strains, involving release of potential energy over distances of the order of a hundred miles or even more. The principal fracturing runs its course in a minute or two, but it is an obvious impossibility for large blocks of the earth's crust to readjust completely in so short a time. The readjustment continues over a period of hours, days, or months, by a process closely allied to what is known in testing laboratories as elastic afterworking. The process is not continuous, because of the frictional resistance to displacement along the original line of fracture; local strains due to afterworking must rise to a certain threshold level before the original displacements can continue. The magnitude scale makes it possible to estimate the strain released in each minor event and to plot the strain release against time. In this way curves are obtained analogous to those observed in laboratory specimens. It is possible to estimate the maximum magnitude of an expectable aftershock during the process of any given sequence, and to decide whether a large following shock is a true aftershock or represents the beginning of a new event.

Markus Båth has found that in many aftershock sequences the largest aftershock has a magnitude about 1.2 units lower than that of the principal shock. Deviations from this occur in identifiably abnormal sequences, usually characteristic of particular regions. Such sequences include earthquake "swarms" such as are common in the region of Imperial Valley, California—sequences of earthquakes, small and large, with no one outstanding principal shock. Such swarms appear to be commonest in areas of structural complexity, where the crust is broken up into blocks of comparatively small size. Earthquake swarms are common in volcanic regions, especially in association with eruptions; here the fundamental mechanism is of another sort, attributable to subterranean movements of molten rock.

It was noted that small shocks contribute relatively little to energy release. In terms of strain release, the relationship is less definite; thus, the strain released in a series of aftershocks may be comparable to that occasioned by the main shock. This is due to the fact that strain release increases with the square root of the energy. Benioff has pointed out that, on this basis, small earthquakes may operate to delay or inhibit the accumulation of a major strain and so act to postpone a large earthquake. However, once a large strain has actually accumulated, the "square" factor comes into play; a major strain can be released only in a major earthquake, or in a great number of small shocks that greatly exceed in frequency those normally observed in a given seismic region. The idea, born chiefly of wishful thinking, that small earthquakes may act as a "safety valve" is thus justifiable only in a restricted way and should not be allowed to affect deliberations in connection with precautionary measures and regulations.

Notes and Bibliography

For additional discussion and references, see C. F. Richter, *Elementary Seismology* (Freeman, San Francisco, Calif., in press).

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On "The New Cell Theory"

Two Soviet authors critically review recent Soviet work on the origin of the cell.

L. N. Zhinkin and V. P. Mikhaĭlov

The discovery of the cell and the formulation of the cell theory were considered by F. Engels to constitute one of the most important stages in the development of natural science. Since the time of the creation of the cell theory, more than a century has passed. During that period the cell theory, constituting one of the fundamental elements in contemporary biology, has had its meaning intensified and enriched by a mass of new facts.

Quite recently the cell doctrine has been subjected to radical revision by O. B. Lepeshinskaya, who, on the basis of her own experimental findings, has built up a "new dialectic-materialist cell theory," in which it is asserted that every cell develops from living substance.

The views of O. B. Lepeshinskaya received complete approval at the conference on the problem of living substance and cell development of the Division of Biological Sciences of the Academy of Sciences of the U.S.S.R., held 22–24 May 1950. The conference carried a resolution in which it was affirmed that "the works of O. B. Lepeshinskaya and her co-workers opened a wide avenue for the investigation of acellular forms of life and the most delicate processes of cell development within and outside the organism. . . . The conceptions developed by O. B. Lepeshinskaya must be widely publicized and must be used to the utmost advantage in the practice of medicine and agriculture" (*1*). By a decision of the Presidium of the Academy of Medical Sciences of the U.S.S.R. of 14

May 1950, revision of the timetables of higher educational establishments and of the contents of textbooks in the light of the "new cell theory" was foreshadowed. The findings of O. B. Lepeshinskaya, as if proved absolutely, were explained in a number of books and brochures. They were inserted in higher education textbooks on biology, histology, and cytology, and in the school textbook, *Essentials of Darwinism*. Special works were devoted to explanation of the philosophical significance of the "new cell theory."

It should be mentioned that from 1950 the development of the "new cell theory" was proceeding in the unhealthy atmosphere of excessive general adulation. The theory as a whole was not assessed critically. There was no expert discussion of questions of cytology. More than this, all who earlier had come forward with criticism of the views of O. B. Lepeshinskaya were reckoned among the reactionaries in science, among the metaphysicists, idealists, and so on. And so it was forgotten that no science can develop without conflict of opinion, and that critical appraisal of the views advanced by O. B. Lepeshinskaya was the more necessary in that her findings contradicted a great many well-established facts which were almost completely ignored by her and her commentators.

Recently a number of works have been published describing the results of control examinations of the experimental material of O. B. Lepeshinskaya and her co-workers. An analysis of these works is very essential to an assessment of the theory as a whole. It is exactly such an analysis that we shall try to make in this article.

Chick Embryos

In her study of the development of the chick embryo, O. B. Lepeshinskaya described the development of entodermal cells and blood islands from the yolk spheres (*2, 3*). The formation of crystal-like structures which, through a number of stages, became reorganized into "true living cells," was described by O. B. Lepeshinskaya in cultures of the white of hen egg (*4*). Recently certain facts have emerged which compel the belief that these descriptions of the formation of cells from the yolk and white of hen egg are erroneous. V. N. Orekhovitch, M. I. Leviant, and T. P. Levchuk-Kurokhtina have published the results of their work with the inclusion of labeled amino acids in the proteins of the albuminous envelope and yolk of the incubated hen egg (*5*). They demonstrated quite convincingly that in the process of incubation of the fertilized hen egg and of embryo development, no new regeneration of the proteins of the albuminous envelope and yolk occurs. The labeled amino acids were, however, observed in the albuminous envelope and yolk of the egg, when they were introduced into the body of the hen while still carrying the egg, that is, at the time when it was being formed in the egg tube. On the basis of their findings that, during the period of incubation, dynamic exchange between the proteins of the albuminous envelope, the embryonic disc, and the yolk is absent, the authors drew the conclusion that "the hypothesis on the possibility of the development of cell elements from the white and yolk of hen egg would appear to be improbable" (*5*, p. 612). A direct reexamination of O. B. Lepeshinskaya's data on the development of cells from the yolk spheres has been carried out by A. G. Knorre by various cytological and histochemical methods and by ultraviolet microscopy. According to his findings, "the yellow and white yolk spheres of the hen egg are not moner-like living structures, and are not transformed into cells" (*6*).

A reexamination of these experiments of O. B. Lepeshinskaya was carried out over a period of two years at the Biological Institute of the Academy of Sciences in Czechoslovakia. The Czechoslovak scientists "carefully reproduced the conditions of the experiment as described by O. B. Lepeshinskaya, but were only able to follow differentiation in the yolk to the stage of yolk spheres, while in no single instance was the formation of cells from the spheres observed" (*7*).

Blood Plasma

M. G. Kritsman, A. S. Konikova, and Ts. D. Osipenko, employing the method of labeled amino acid incorporation, observed vital regeneration of the proteins in blood plasma freed from cells (*8*). These data were used by adherents of the "new cell theory" as confirmation that the blood plasma is one of the variants of the acellular living substance.

On the strength of these data, M. D. Skobel'skii, working in the laboratory of O. B. Lepeshinskaya, investigated the progress of visible structural changes in the cell-free plasma of the hen. He described peculiar structures ("plasmospheres") which were formed in the plasma during incubation (*9*). The "plasmospheres," in the view of M. D. Skobel'skii, should be regarded as "representatives of the living world." O. B. Lepeshinskaya considered that the "plasmospheres" represented cells being formed from the living substance of the plasma (*10*, p. 15).

A reexamination of the findings of M. G. Kritsman, A. S. Konikova, and Ts. D. Osipenko was carried out by V. N. Orekhovitch, T. P. Kurokhtina, and N. D. Buianova with labeled amino acids; this demonstrated that, under sterile conditions, vital regeneration of the proteins of incubated plasma did not occur (*11*). In other words, in the plasma there is no metabolic exchange, and to regard it as the representative of living substance is impossible.

A check of M. D. Skobel'skii's data was made by G. N. Voronin and V. P. Mikhaĭlov (*12*). They confirmed that "plasmospheres" are formed in hen blood plasma during incubation. But, according to their findings (the work was carried out in consultation with the crystallography specialist, S. M. Ansheles), the "plasmospheres" were radially arranged aggregates of acicular crystals (spheroliths), the process of formation and growth of which proceeds in exactly the same way as has been established for all crystals. "There is little serious evidence on which it is possible to approximate 'plasmospheres' to 'representatives of the living world,' and none to justify regarding them as 'cell-like structures,' still less as cells" (*12*, p. 631).

Hydras

O. B. Lepeshinskaya's experiments with mutilated hydras played a very important part in the creation of the "new cell theory." A repetition of these experiments was carried out by V. E. Kozlov and P. V. Makarov (*13*). In the course of the investigation on mutilated, living hydras, the appearances which were regarded by O. B. Lepeshinskaya as constituting the new formation of cells and their subsequent conversion into cell conglomerates (moruloid stage) were reproduced. Essentially the same result, however, was obtained by these investigators with ground, fixed hydras. Consequently there are no grounds for speak-

The authors are on the staff of the Moscow Oblast Scientific Research Institute of Obstetrics and Gynecology. This article was first published in *Arkhiv Anatomii, Gistologii i Embriologii* [32, No. 2, 66 (1955)]. It was translated by the Pergamon Institute for the Russian Scientific Translation Program of the National Institutes of Health, U.S. Public Health Service.

ing of the isolation from hydras of living substance and its subsequent transformation. In no single one of the experiments was anything resembling cells observed, and the authors speak rather of progressive changes in the spherical structures, their shriveling, vacuolization, and so forth (*13*, pp. 58, 59). The authors conclude that the plastic processes observed by O. B. Lepeshinskaya and by them in the substance separated from the hydras were in the category not of biological but of physicochemical processes. A repetition of the Lepeshinskaya experiments on the formation of cells from mutilated "living substance" of the hydra was also made in Czechoslovakia in the laboratory of F. Grechik, and gave negative results (*7*, p. 1433).

Sturgeon Eggs

According to O. B. Lepeshinskaya, the eggs of the sturgeon during the period of their maturation lose the nucleus, which later is again formed afresh from the cytoplasmic granules. The ova without nuclei pass through a stage of acellular structure; the process of formation of the female pronucleus, as represented by the author, reflects stages in the phylogenetic development of the cell from the acellular living substance. These observations of O. B. Lepeshinskaya were carefully reexamined by B. N. Kazanskii (*14*) and by T. I. Faleeva (*15*). The latter showed that, in the egg cells of the stellated sturgeon and the sturgeon, the nucleus, in one of its own several stages, is present at all periods of their development. Every roe was examined by her in a series of more than 200 sections, 7 mμ in thickness, and only in one or two of them was the nucleus observed to be of small size.

B. N. Kazanskii, who made a detailed examination of the processes of egg cell maturation, ovulation, and fertilization in sturgeon, showed that the nuclei in the ovocytes maintain their line throughout the period of maturation.

The investigations of B. N. Kazanskii and of T. I. Faleeva have revealed an error on the part of O. B. Lepeshinskaya (*16, 17*).

Connective Tissue Cells

The new formation of connective tissue cells from acellular living substance (with a fine granularity, formed as a result of the breakdown of rich cells which have phagocytized "blood granular substance") was described by O. B. Lepeshinskaya in relation to the healing of skin wounds in mice (*3*). Developing her hypothesis, L. V. Polezhaev tried to prove that the cells of the regenerated blastema arose without mitotic or amitotic multiplication, as a result of accumulation from undifferentiated tissues and by neoformation from acellular living substance in the region of the wound (*18*). It must be emphasized that the descriptions by O. B. Lepeshinskaya are by no means convincing. She saw, in fixed, stained preparations, in a microscope field, granules of various sizes, some sort of degenerating cells, and cells of lymphocyte type. Such pictures [for example, Fig. 24, Table 29, of her monograph (*3*)] will convince no one that here a process of cell formation is actually taking place. With considerable justification they could be regarded as various stages in a process of cell breakdown. O. B. Lepeshinskaya understands this excellently herself. It is for this reason that she cautiously writes that the conclusion as to the development of connective tissue cells in the wound from acellular living substance has been formed by her as a hypothesis, based on indirect considerations (findings on the formation of cells from cytoplasmic spherules separated from disrupted cells of the hydra; a sharp increase in the number of cells in an inflammatory focus unaccompanied by any significant number of mitotic figures).

"Such a hypothesis," she writes, "must be put forward in view of certain facts known to us, and we must try by all the most modern investigational means to confirm it" (*3*, p. 169). Ultimately, without carrying out the supplementary experiments which she had mentioned, O. B. Lepeshinskaya altered this cautious formulation to a categorical statement (*19*, p. 133).

The findings of L. V. Polezhaev were reexamined by V. P. Mikhaĭlov (*20*), who showed that in the regeneration blastema which formed after removal of the tail of a tadpole (L. V. Polezhaev had worked with tadpoles) there were numerous mitoses at all stages of the regeneration. In the fibrin covering the stump, degenerating elements and granules of varying size could be seen together with cells of normal appearance. All these elements can, at will, be arranged in an unbroken series. To reach the conclusion, however, on the basis of the appearances observed (in exactly the same way as in the case of the appearances described by O. B. Lepeshinskaya during regeneration of the skin in mice), that in such a case the development of cells from living substance of acellular structure is going on, is hardly possible. From an analysis of the data of O. B. Lepeshinskaya and L. V. Polezhaev, it is right to reach the conclusion that "as yet there are no convincing proofs of the neoformation of cells, during regeneration, from acellular living substance" (*20*, p. 55).

N. N. Anichkov, Iu. M. Zhabotinskii, and T. A. Sinitsina made a special study of the process of origin of cells and fibers in rabbits in the course of aseptic inflammation (*21*). The work was carried out, just as was that of O. B. Lepeshinskaya, on the regeneration of the skin, with fixed material. The authors "failed to obtain any clear proofs or evidence in favor of the origin of any types of cell elements from any extracellular formations" (*21*, p. 42). They emphasized that the ordinary current methods of cytological examination of fixed preparations were unsuitable for the solution of this problem.

Conclusion

Thus, the basic material underlying the "new cell theory" does not stand reexamination. The "new cell theory" is not founded on solid, firmly established facts and, consequently, does not reflect any laws actually existing in nature. The hypotheses advanced by O. B. Lepeshinskaya can only claim to depict the personal, subjective views of her and her supporters.

We have been obliged to come to this conclusion despite the fact that in 1950 a number of authoritative scientists acknowledged the factual material on which the "new cell theory" was based as authentic.

From lack of space we cannot dwell on the numerous works published since 1950, the authors of which were endeavoring, with different types of experimental material, to demonstrate the development of cells from acellular living substance. This has been done by us to a certain extent in another paper. Some of these works have been executed at an unusually low technical level. The authors, starting with a preconception, arbitrarily treat the various visible stages in the process of cell breakdown in fixed and stained preparations as stages in their new formation. In most works the conclusion on the development of the particular type of cell from acellular living substance is drawn with reservations, and in conjectural form ("appar-

The following excerpt from the Soviet Monitor *of 4 June 1950 is pertinent as background information. The English translation is reprinted, with permission,* from Occasional Pamphlet No. 10 *of the Society for Freedom in Science, dated November 1950. For the work described here, Professor Lepeshinskaya received a Stalin prize, first class.*

New Research Work on the Origin of the Cell

Olga Lepeshinskaya, professor of biology, is conducting interesting research on how the cell originated from living matter and on the part played by such matter in the organism. This research work of the Soviet scientist marks a new stage in providing a materialist theory of the cell and opens up broad prospects for establishing the principal laws of the organic world.

Lepeshinskaya has succeeded in proving that the formation of new cells in the living organism occurs not only through multiplication of cells themselves but also through their development directly from so-called noncellular matter, which is always present in the organism.

A few days ago Professor Lepeshinskaya reported on her work at a conference at the U.S.S.R. Academy of Sciences. She proved the possibility of the development of biological organisms and cells from the nonstructural albumen of eggs of various birds. This research proved that egg albumen is not merely a lifeless nourishing medium, but living matter capable of developing and forming cells.

The work carried out by Professor Lepeshinskaya and her colleagues has introduced many new elements into the study of the properties of living matter and has evoked the lively interest, and earned the acclamation, of prominent Soviet scientists who have noted the great significance of these researches for biology and medicine.

Thus Academician Trofim Lysenko said that the data obtained by Professor Lepeshinskaya in studying the origin and development of the cells of living matter constituted a major contribution to the development of the theory of Soviet Michurin biology. They help to gain a correct understanding of the phenomena of new formations in the organic world, to comprehend and to explain the emergence of new kinds of organisms within the old species.

Professor Lepeshinskaya's work was also highly assessed by Academicians Alexander Oparin, Evgeni Pavlovsky, Alexei Speransky, Nikolai Anichkov, and others. The conference recommended Soviet biologists, agrobiologists, and medical workers to start wide-scale research in the field of the development of living matter and its noncellular form and to utilize in medicine and agriculture the results achieved by Professor Olga Lepeshinskaya and her colleagues.

ently," "produces the impression," "it can possibly be assumed," and so forth).

Undoubtedly life arose in the earth in some simple form and cells were formed as a result of a long process of evolution. The problem of the evolutionary development of the cell is by no means a new one. It has already been broadly stated by the well-known protagonist of Darwinism, Ernst Haeckel. Its great importance was excellently understood by the leading Russian scientists in the '70's of the last century. A. E. Golubev, a professor at Kazan University and at the Petersburg Medicosurgical Academy, starting from the evolutionary hypotheses developed by Haeckel and his own experimental findings, had stated already in 1874 that "the study of the properties of protoplasm at its various levels of development is a matter of prime importance. Unfortunately, our knowledge in this respect is extremely scanty" (*22*, p. 8).

It is certain that, once formed, cells have traveled a long path of evolutionary development (this can be seen even in a comparison of the cells of algae and those of the higher plants). Moreover, as a result of this evolution, structures of a higher order (various symplasts, complex systems of cells and acellular material combined with them) are formed in the body of multicellular organisms. Obviously, in the complex, complete organism with its nervous integration the cells are not independent elements, as Virchow and Fervon represented them to be. From all this, however, it does not follow that the process, accomplished formerly, of the development of cells from the primitive living substance is still being effected among the representatives of the animals and plants, and that every cell, in the course of its own development, must pass through the infinitely remote acellular living substance stage. This, however, is what O. B. Lepeshinskaya has postulated, not on grounds of firmly established facts, but on speculative hypotheses, linked with a quite primitive understanding of biogenetic law.

A. A. Zavarzin, D. N. Nasonov, and N. G. Khlopin, who published a criticism of the views of O. B. Lepeshinskaya in 1939, drew attention to this point (*17*). Despite the fact that these authors had not at their disposal any factual material by which to check O. B. Lepeshinskaya's findings, their criticisms and general appraisal of the "new cell theory" were, in our opinion, fundamentally justified.

F. Engels, speaking of natural philosophy, pointed out that it "substituted links fashioned of ideas and fantasy for the true, but still unknown, connections between phenomena, and replaced inadequate facts by inventions, filling the actual gaps only in imagination" (*23*). The "new cell theory" is a classic example of natural philosophy teaching (*24*).

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16. O. B. Lepeshinskaya, in her monograph (both in the first and in the second, amended and amplified edition), quotes from the well-known work of V. V. Zalenskii (1878), who described the disappearance of the nucleus in the eggs of sterlets some time after spawning. This quotation, to which attention has already been drawn by A. A. Zavarzin, D. N. Nasonov, and N. G. Khlopin (*17*), is not accurate. V. V. Zalenskii, in his work, while not speaking in terms of karyokinesis and reduction division, which, at the time, he did not know about, describes the picture of mitotic division with disappearance of the nucleus ("germ vesicle") and the appearance of chromosomes ("fine spaces of varying shape, filled with a homogeneous substance, staining well with hematoxylin, and closely resembling the substance of the former germ vesicle").
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24. A number of problems connected with the "new cell theory" have not been touched on in this paper. A selection of some of these questions, together with a critical analysis of a number of morphological works in which the authors by various methods have tried to demonstrate the neoformation of cells from "acellular living substance," are to be found in our paper, "The new cell theory and its proof," *Uspekhi Sovremennoĭ Biol.* 39, 228 (1954).

Chromosome Studies of Primates

The application of new culture and cytological techniques should help solve some puzzles of evolution.

Michael A. Bender and Lawrence E. Mettler

Although the evolution of the primates is a subject which has long interested biologists, it is only relatively recently that any attempt has been made to determine their relationships through a study of their chromosomes. The main reason for this situation is the notorious difficulty of studying mammalian material, which is amply illustrated by the long dispute over the chromosome number of man. It is difficult to make accurate counts by means of the standard techniques of sectioning or by making smears of tissue removed at necropsy, while to obtain detailed karyotypes from such material is virtually impossible.

The recent development of new techniques for the culture of diploid somatic cells in monolayers on glass surfaces, as well as the development of new cytological techniques, has made it possible to determine not only the chromosome numbers but also the chromosome morphology of a great variety of animals which have not been previously studied. Making use of such techniques, Hsu (*1*) and Tjio and Levan (*2*) have published detailed karyotypes of man, while Chu and Giles (*3*) have recently published chromosome counts for five genera of catarrhine or Old World monkeys. Only one count for a platyrrhine or New World monkey is available (*4*), and so far as we know there have been no counts reported for any prosimian primate.

Karyotype analyses of other groups of animals and of plants, such as those of Patterson and Stone (*5*) on the genus *Drosophila* and those of Babcock (*6*) on the genus *Crepis*, have been extremely successful in elucidating the evolution of these groups. Within the order Primates there exist many cases in which the classical anatomical approach has been unable to clarify relationships. An example is the quandary in which taxonomists find themselves when attempting to classify the genus *Callimico*. Various authors have assigned this genus to the family Cebidae or to the family Hapalidae, or have erected a new family, Calimiconidae, to contain it. With the hope that a cytological survey might clear up such puzzles, we have undertaken to investigate the order Primates, paying special attention at present to the infraorder Platyrrhina. The work discussed here (*7*) includes study of one prosimian, three genera of Catarrhina, and four of Platyrrhina.

Materials and Methods

Primate material has been obtained from several sources. Recently dead animals have been obtained from the Bal-

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timore Zoo (*8*). Many live specimens, especially of the rarer genera, have been obtained from dealers (*9*). In all cases the tissue cultures were made from kidney. This was done mainly because this organ can easily be removed from the living animal, which can then be used for other purposes.

The tissue was prepared for culture by the trypsin digestion technique of Younger (*10*). Mass cultures were made in a modified Chang's medium (*11*) consisting of Hanks' balanced salt solution (70 percent), adult human serum (25 percent), and beef embryo extract ultrafiltrate (5 percent). This medium also contained 100 units of penicillin, 0.1 milligram of streptomycin, and 2.5 units of Mycostatin per milliliter. The kidney cells cultured in this medium were predominantly epithelioid. They were maintained in serial culture for a maximum of three passages. For the preparation of a new passage, and also for the preparation of cover-slip cultures, the cells were digested free from the glass surface and from each other with 0.05-percent 1:250 trypsin solution made up in Puck's "saline A" (*12*). The resulting cell suspension was added to fresh culture medium and placed in appropriate culture vessels. For cultures intended for cytological examination, the cover slips were placed in the bottom of 1-ounce ointment jars with plastic screw caps. These cultures were maintained in an atmosphere of 5 percent CO_2 in air. The usual initial inoculum was about 2×10^4 cells in 5 milliliters of medium per ointment jar.

When a relatively confluent sheet of cells had grown on the cover slips (this usually took about five days), the cultures were used for cytological preparations. Two days before the cells were to be fixed, the old medium was replaced with fresh medium. This procedure usually results in an increase in mitotic activity after a lag period of 24 to 36 hours. Ten to fifteen hours before fixation, colchicine was added to some of the cultures to yield a final concentration in the medium of $10^{-7}M$. The hypotonic pretreatment method of Hsu and Pomerat (*13*) was used for all preparations. Ten to twenty minutes before the cells were to be fixed, the medium was replaced with a 20-percent balanced salt solution in water. The cells were fixed in Darlington and LaCour's 2BD. After washing they were stained by the Feulgen method. The cover slips bearing the stained cells were inverted on slides in 45-percent acetic acid and flattened with moderate pressure in a bibulous book. The slides were dehydrated by the Dry-Ice technique of Conger and Fairchild (*14*) and mounted in euparal.

Table 1. Somatic chromosome numbers and chromosome types of eight genera of primates. The classification used here is that of Fiedler (*19*). M, metacentric; S, subterminal; T, telocentric.

Infraorder	Scientific name	Common name	Sex	Chromosomes					
				2*N*	Autosomes			X	Y
					M	S	T		
Lorisiformes	*Nycticebus coucang*	Slow loris	♀	50					
Catarrhina	*Cercopithecus mona mona*	Mona guenon	♂	66			6		
Catarrhina	*C. mona mona*	Mona guenon	♀	66			6		
Catarrhina	*Cercocebus torquatus torquatus*	Sooty mangabey	♂	42			0		
Catarrhina	*Papio sphinx*	Mandrill	♂	42			0		
Platyrrhina	*Cebus apella*	Cinnamon ringtail	♂	54	6	18	28	T	T
Platyrrhina	*C. apella*	Cinnamon ringtail	♂	54	6	18	28	T	T
Platyrrhina	*C. capucinus*	Capuchin ringtail	♀	54	6	18	28	T	
Platyrrhina	*Callicebus cupreus*	Red titi	♂	46	10	10	24	S	T
Platyrrhina	*Saimiri sciureus*	Squirrel monkey	♂	44	16	14	12	S	T
Platyrrhina	*S. sciureus*	Squirrel monkey	♀	44	16	14	12	S	
Platyrrhina	*S. sciureus*	Squirrel monkey	♀	44	16	14	12	S	
Platyrrhina	*Ateles geoffroyi cucullatus*	Hooded spider monkey	♂	34	12	18	2	M	T
Platyrrhina	*A. paniscus chamek*	Black-faced spider monkey	♂	34	12	18	2	M	T
Platyrrhina	*A. paniscus chamek*	Black-faced spider monkey	♀	34	12	18	2	M	

More than 30 good figures were scored for each species, except for *Cebus capucinus* and *Ateles geoffroyi chamek*, in which 25 cells were scored for each species. In no case were less than 12 cells scored for each individual. Freehand drawings were made of all cells to facilitate counting. Some normal and some colchicine metaphases were used for each individual. Wherever available, many slides from all three passages were examined. In order to minimize the chance of counting figures from which some of the chromosomes had escaped, most counts were made on unbroken cells. The figures to be counted were preselected at low magnification for ease of counting. In most instances those cells which were judged before counting to be the best gave completely consistent counts. In spite of this preselection, however, some cells were scored in which the chromosome number could not be determined exactly. Later karyotype analysis allowed most of these cases to be resolved. Except for tetraploids and a few cells which seemed to be the product of tripolar division of tetraploids, the chromosome counts showed very little variation. It is our opinion that there is no real variation in chromosome number in diploid cells from our material. The few counts which differed from those of the modal class were of cells in which some chromosomes overlapped. It is difficult, if not impossible, to be certain of the chromosome number in these cases.

A number of camera lucida drawings were made from each platyrrhine species. The figures used for this purpose were usually colchicine metaphases and were frequently from cells which had broken during the flattening process. Such well-spread figures were found to be the most useful for determining the position of the centromeres. Only those colchicine metaphases were used in which the chromosomes had contracted to about the same degree as those of normal metaphases. Karyotypes were prepared by tracing the chromosomes from the camera lucida drawings. Where possible they were paired according to size and centromere position, but in some instances groups of four or more similar chromosomes had to be paired in arbitrary fashion. Photographs were also made (*15*).

Results

Fifteen individuals were analyzed for chromosome number. These are listed in Table 1, together with the diploid chromosome number. In order to test the possibility that a difference exists between the chromosome complements of cells grown in vitro and those growing in vivo, the bone marrow of the *Ateles paniscus chamek* male was examined. A

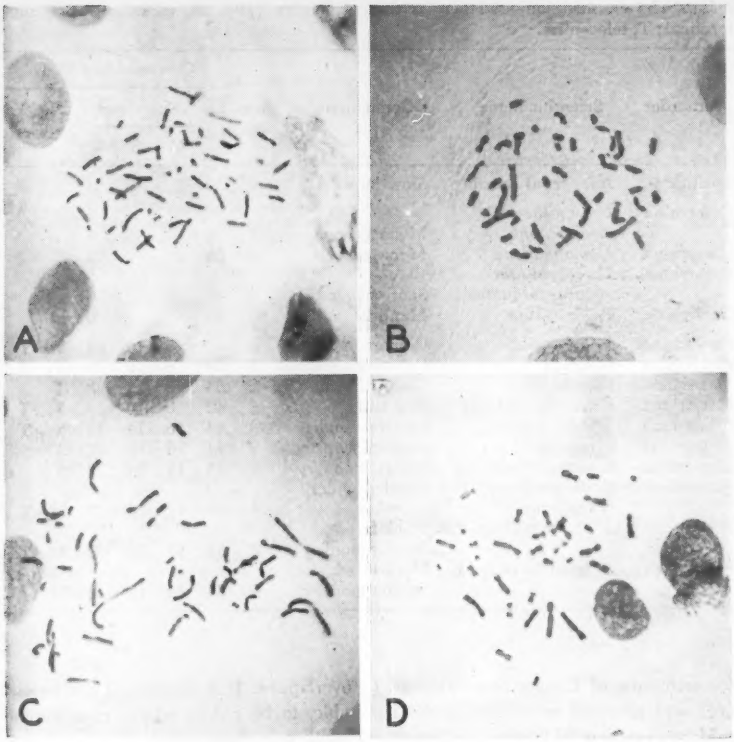

Fig. 1. Diploid figures from tissue-cultured material treated with colchicine. (*A*) *Cebus apella* ♂; (*B*) *Callicebus cupreus* ♂; (*C*) *Saimiri sciureus* ♂; (*D*) *Ateles geoffroyi cucullatus* ♂.

modification of the technique of Ford and Hamerton (*16*) was used. The counts obtained from this material verified the diploid condition (and the chromosome morphology) of the vast majority of the tissue-cultured cells. Most cells not conforming to the diploid condition were tetraploid. An extremely low number of cells with higher ploidy were present, while aneuploidy was not observed. Photographs of representative diploid cells from the four platyrrhine genera are presented in Fig. 1.

The chromosome number listed in Table 1 for the slow loris (*Nycticebus coucang*) is particularly significant, since it represents the first report on a prosimian primate. Unfortunately, the material was such that a detailed karyotype analysis was not possible.

The three genera of the family Cercopithecidae (Old World monkeys) which are reported here have recently been investigated by Chu and Giles (*3*). All are members of the subfamily Cercopithecinae. The chromosome number 66 for *Cercopithecus mona mona* confirms Chu and Giles' count for a different subspecies of this form. Likewise, the count of 42 for *Cercocebus torquatus torquatus* is the same as that reported by Chu and Giles for a different subspecies. The count for *Papio sphinx* is the first for this species, although the number 42 has been reported for other members of this genus by Chu and Giles and also by Darlington and Haque (*17*).

A detailed karyotype analysis was made of four genera of Platyrrhina (New World monkeys), all members of the family Cebidae. A representative male karyotype for each genus is presented in Fig. 2. The chromosomes have been classified into three morphological categories: telocentrics (T), subterminals (S), and metacentrics (M). Chromosomes with approximately equal arms have been designated as metacentric, while those with unequal arms are classified as subterminals. Chromosomes with no visible second arm are considered telocentrics. The autosomes of the four genera have been classified into these three categories in Table 1. The sex chromosomes have been classified separately. No autosomal difference was found either between individuals of the same species or between species of the same genus. In the genus *Saimiri*, a prominent constriction occurs in the centromere region of one metacentric pair, while in *Ateles* one metacentric pair has a prominent secondary constriction. The X chromosome varies all the way from a terminal (in *Cebus*) to a metacentric (in *Ateles*). The Y chromosome, which is the smallest chromosome in all four genera, appears to be a telocentric.

All of the figures used in preparing the karyotypes were colchicine metaphases, which were selected so that the amount of contraction was about the same as that at full metaphase in a normal mitosis. Although the four karyotypes are of the same magnification, the over-all size difference is not necessarily indicative of the natural state. Absolute and relative lengths can vary from cell to cell. For example, the absolute size of the largest telocentrics in the *Callicebus* material ranged between 6.8 and 8.0 microns. The relative lengths of the two largest pairs in *Callicebus* differ to the extent that in a few cells the second, or subterminal, pair is longer than the first, or telocentric, pair. Occasionally a size difference exists between the homologs, especially among the very long chromosomes. Thus, in the cell used for the *Saimiri* karyotype, the two members of the second pair of chromosomes are not of the same length. It is believed that this is the result of differential stretching due to the spreading technique used.

Discussion

The results of the present study, together with those of Chu and Giles (*3*), reveal a large variation in chromosome number among representatives of three major groups of primates. The numbers range from $2n = 34$ to $2n = 66$, and in such a way that their only common denominator is 2. This fact makes it very unlikely that polyploidy could have been a factor in the evolution of the primates; this is not surprising, since polyploidy must be an extremely rare evolutionary mechanism in species, such as those recorded here, which have a well-differentiated pair of sex chromosomes.

There are several other mechanisms which can lead to an evolutionary change in chromosome number. Of these, fusion has been most clearly demonstrated. Patterson and Stone (*5*), for instance, have suggested that centric fusions account for the reduction in chromosome number and the origin of metacentric and subterminal chromo-

somes in the genus *Drosophila*, in which the most primitive species are characterized by rod-shaped chromosomes. Makino (*18*) showed that the chromosome number of the domestic sheep is 54, and that, of these chromosomes, six are metacentrics, the rest telocentrics. He found that the goat, on the other hand, has 60 telocentric chromosomes. Makino suggests that 12 of the chromosomes of the goat correspond to the 12 arms of the six metacentrics in the sheep.

The four karyotypes presented here for the family Cebidae strongly suggest that centric fusion has played an important role in the evolution of this group. Of the four genera, *Cebus* is morphologically the least specialized, while *Ateles* is obviously highly specialized for an arboreal existence, having a well-developed prehensile tail, a shortened trunk, elongated limbs, and (usually) no thumb. The other two genera, *Callicebus* and *Saimiri*, are both moderately specialized for a squirrel-like existence, although they are not considered to be very closely related to each other.

The four genera thus form a series of increasing specialization. The chromosome numbers of these genera show a correlation with their degree of specialization. It is clear from Table 1 that a reduction in chromosome number is correlated with a relative decrease in the number of telocentric chromosomes. This is expected if the reduction in number has been accomplished by means of centric fusions. The karyotypes strongly suggest that *Cebus*, with 14 pairs of telocentric chromosomes, is the most primitive of the four genera studied, although this in no way implies that *Cebus* is ancestral, or even that this genus is the most primitive in the living Platyrrhina.

It is obvious that other mechanisms of karyotype evolution must also have been important in the Cebidae. Tandem fusions and pericentric inversions must have occurred to produce the longer chromosomes in each set, particularly chromosomes like the longest pair in *Callicebus*. Such processes would account for the difference in karyotype between the genus *Callicebus* and the genus *Saimiri*. It is interesting to note that if one arbitrarily decides how many small telocentrics it would have taken to make each chromosome in each of the four genera, the total for each genus is close to 70. A plausible speculation, therefore, is that the basic primitive karyotype for the Platyrrhina, and perhaps even for the order Primates, might have been about 70 small telocentrics.

Several features of the karyotypes for the Cebidae are of particular interest. While the Y-chromosome has the same morphology in each of the genera, the X-chromosome, which is a simple telocentric in *Cebus*, is a subterminal chromosome in *Callicebus* and *Saimiri* and a metacentric in *Ateles*. Constant features of the cebid karyotype seem to be a small metacentric pair, which is the next-to-last pair in all of the genera but *Cebus*, and a small telocentric pair which is present even in *Ateles*. A pair of chromosomes with a prominent secondary constriction, which has been reported by Chu and Giles (*3*) to be a constant feature in the Cercopithecidae, is not present in all of the Platyrrhina investigated, although the present study confirms its presence in the genera *Cercocebus*, *Papio*, and *Cercopithecus*. Neither *Cebus* nor *Callicebus* possesses a pair of chromosomes with such a constriction, while the constriction in *Saimiri* does not appear to be secondary. It is possible, of course, that if the constriction noted by

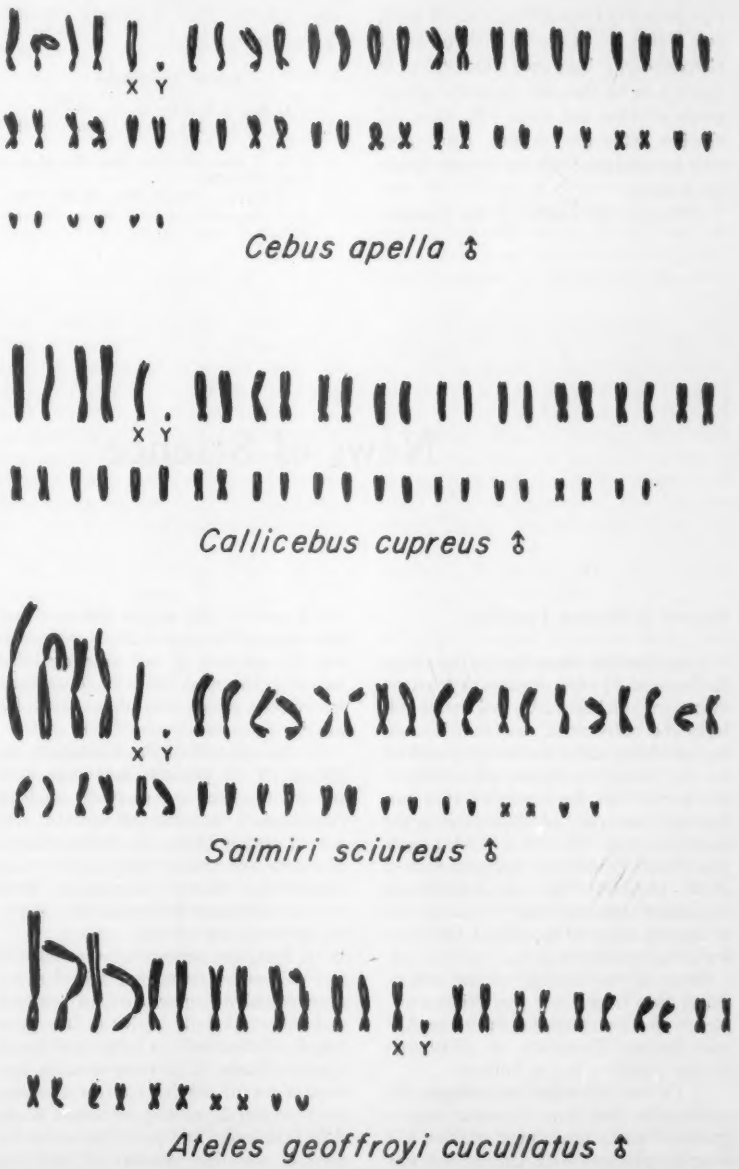

Fig. 2. Mitotic chromosomes of representative species of four platyrrhine genera. The chromosomes are arranged by paired homologs and in order of size.

Chu and Giles is the nucleolus organizer, it may exist as an undetected second arm in one of the telocentric chromosomes of *Cebus* and of *Callicebus*.

The mechanism of chromosome evolution suggested for the Cebidae does not apply in any obvious way to the Cercopithecidae. If centric fusions have any importance in the latter family, the evidence has been obscured by further specialization of the karyotypes through such mechanisms as pericentric inversion. It is significant, however, that while the genera *Papio* and *Cercocebus* do not appear to have any telocentric chromosomes, there are three pairs of telocentrics in *Cercopithecus mona mona* (see Table 1). If the $2n = 60$ species of *Cercopithecus* have no telocentrics, as appears to be the case from the photograph of Chu and Giles (*3*), then the number difference in this genus may well be explained by the centric fusion mechanism.

Although the studies of the chromosomes of the Primates which have been made to date have only scratched the surface, so to speak, it is already obvious that such studies can be of great help in the analysis of the problem of the evolution of this group. Studies are now in progress in our laboratory on the chromosome numbers and karyotypes of a second family of the Platyrrhina, the Callithricidae. Preliminary work is also in progress on the rather puzzling genus *Callimico*. It is hoped that these studies will both clear up the question of the taxonomic position of *Callimico* and answer the question of whether the Callithricidae are truly primitive primates or have evolved their seemingly primitive characters secondarily.

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8. We are indebted to Mr. Arthur Watson, director of the Baltimore Zoo, for his kind cooperation.
9. Miss Trudie Jerkins, of the Tarpon Zoo, Tarpon Springs, Fla., has been extremely helpful in obtaining New World monkeys for this study.

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News of Science

Survey of Physics Teaching

A nationwide survey by the American Institute of Physics discloses a shortage of physics teachers in United States colleges and universities, overloaded teaching schedules, and a discouraging outlook for the immediate future. One result of the survey was the revelation that college and university administrators in the academic year 1957–58 provided sufficient funds for 403 new appointments of Ph.D. physicists, but the departments concerned estimated they would be able to appoint only 254 new Ph.D. physicists from the available supply.

Some of the findings of the survey, which was conducted by William C. Kelly, director of education at the AIP and former University of Pittsburgh faculty member, are as follows:

1) Of the 536 American colleges and universities that have a 4-year undergraduate major program in physics, 490 —or 91 percent—took part in the survey. Some 451 institutions reported that their needs for physics teachers are not being met in some degree and that they have had to resort to various substitutes for the services of full-time qualified teaching personnel. Only 39 educational institutions report that their needs for physics teachers are now being met.

2) Almost half of the institutions replying, or 49 percent, said that their physics teachers are carrying teaching "overloads." Another 30 percent reported that graduate or undergraduate assistants are being relied upon to an "undesirable degree" in teaching. Most of these assistants have had little previous teaching experience.

3) Forty-six percent of the colleges and universities responding said that the time available to physicists for research and other scholarly activities has been "markedly reduced" as a result of heavy teaching loads. It is recognized as important for all physicists to do scholarly work—research, writing of technical articles and books, and participation in the work of scientific societies—if they are to be effective educators.

4) Class teaching situations need improvement. Twenty-one percent of the physics departments report that they have had to cancel classes because of inadequate staff, another 36 percent report an increase of class size to an "undesirable degree," and one-third state that teaching duties have been assigned to part-time, although qualified, teachers from outside the institution's physics department.

5) Departmental chairmen estimate that approximately 688 Ph.D. physicists are needed to correct the shortages in these colleges and universities. The total number of Ph.D. degrees granted in 1956–57 amounted to 444 in the U.S. More than half of the 444 did not go into teaching because they took full-time research jobs.

6) The situation in the small physics department is disturbing. Half the shortage of physics teachers occurs in physics departments with staffs of six or less people, and half of the bachelor's degrees in physics in 1957 were granted by these same physics departments.

Nuclear Propulsion

A study of the feasibility of employing controlled nuclear explosions for propulsion has been authorized by the Air Research and Development Command, it was announced on 2 July by Roy W. Johnson, director of the Advanced Research Projects Agency. The authorization is for a contract with the General Dynamics Corporation's General Atomic Division, San Diego, Calif.

This is one of a series of investigations involving new means of propulsion for space applications. Conceptually, this study, which was proposed by General Atomic, differs from other proposals under consideration in that it looks to the employment of a series of controlled detonations within the atmosphere and beyond. The initial commitment for this study calls for the expenditure of $1 million during fiscal year 1959. Research will be conducted, in the main, at General Atomic's John Jay Hopkins Laboratory for Pure and Applied Science in San Diego. Continuation of the program will be based upon the results of the study during the initial phase.

Zoological Nomenclature

The International Commission on Zoological Nomenclature has announced that, beginning 6 December 1958, it will start voting on the following cases involving the possible use of its plenary powers for the purpose specified against each entry. Full details of these cases were published on 6 June in the *Bulletin of Zoological Nomenclature* (vol. 16, part 2); (i) *Mysis* Latreille, [1802–1803], designation of type species for, and validation of neotype for species (*Cancer oculatus* Fabricius, 1780) so designated (Class, Crustacea; Order, Mysidacea); (ii) *Dactylioceras* Hyatt, 1867, designation of type species for (Class, Cephalopoda; Order, Ammonoidea: Jurassic); (iii) *gemmascens* Esper, [1794] (*Madrepora*), validation (Class, Hydrozoa; Order, Stylasterina). Comments should be sent as soon as possible in duplicate to the assistant secretary to the commission, R. V. Melville, 28 Park Village East, Regent's Park, London, N.W.1, England.

Oil from Shale

The Denver Research Institute of the University of Denver has announced the successful operation of a pilot plant which extracts oil from oil-bearing shale at a cost which is reported to be commercially competitive. This process, should it prove capable of expansion to industrial scale, would open the nation's oil shale reserves to commercial development and greatly augment our total resources of petroleum.

A reduction in cost of up to 50 percent over other extractive processes has been reported by the institute's director, Shirley A. Johnson, Jr. In a projected source-to-market problem, this reduction would allow a producer to sell Colorado-produced oil on the West Coast for $1.42 to $1.92 a barrel. The current cost of crude oil produced by drilling is about $1.40 per barrel of medium grade, according to a major marketer.

To extract oil from shale by the new process, metal or ceramic balls—thermospheres—are heated and then run counter to a flow of finely crushed shale in a retort. This heats the shale to a point at which it releases its hydrocarbons. The resulting shale coke, which retains some of its combustible elements, is then burned in a furnace to provide heat for warming the thermospheres. This process is one of four considered feasible by experts in the field of shale-oil extraction [*Sci. Monthly* **84**, 275 (1957)].

The pilot plant, which has a daily capacity of 24 tons, has been in operation for the past 9 months. The work is being done by the Denver Research Institute for the Oil Shale Corporation, owner of the Western Hemisphere rights to the process. A Swedish engineer, Olof Aspergen, holds the patent rights on the basic process.

Grants, Fellowships and Awards

General. The closing date for U.S. Government awards for 1959–60 authorized under the Fulbright and Smith–Mundt acts is *1 October*. These grants are for university lecturing and advanced research in Europe, the Near East and Africa, and the Far East. Application forms and additional information are obtainable from the Conference Board of Associated Research Councils, Committee on International Exchange of Persons, 2101 Constitution Avenue, Washington 25, D.C.

Physiological Sciences. The 21st International Congress of Physiological Sciences will be held in Buenos Aires, Argentina, 9–15 August 1959, under the sponsorship of the International Union of Physiological Sciences. The United States adheres to the International Union through the National Academy of Sciences, which has established a National Committee for this purpose. The National Committee is seeking funds to provide a limited number of allotments in support of travel to the congress for scientists residing in the United States who may require such assistance. Individual allotments will not exceed $750 each.

Application for a travel allotment should be submitted in duplicate in the form of a letter giving information on age, training, publications, academic or professional title, and society affiliation. Applicants who plan to submit papers for presentation at the congress should include the proposed title of each paper. Such letters must be submitted *before 15 October 1958* to the Chairman, U.S.A. National Committee on the International Union of Physiological Sciences, 2101 Constitution Avenue, NW, Washington 25, D.C. Applications for assistance toward travel expenses are entirely separate from applications for registration and for the submission of papers.

Poliomyelitis. Deadlines of *1 September* and *1 December* have been established for application to the National Foundation for Infantile Paralysis for postdoctoral fellowships in research and academic medicine or in the clinical fields of rehabilitation, orthopedics, and preventive medicine. Applications for fellowships in the medical associate fields or physical therapy teaching and occupational therapy teaching should also be filed by these dates. For further information write to: Division of Professional Education, National Foundation for Infantile Paralysis, 301 E. 42 St., New York 17, N.Y.

Social science. The Social Science Research Council, 230 Park Ave., New York 17, N.Y., has announced that international conference travel grants equivalent to round-trip, tourist-class fare are offered to social scientists residing in the United States for attendance at meetings outside North America. These grants are offered only for meetings designated in advance by the council.

The tentative list of meetings in 1958 through 1960 follows. The approximate number of grants to be offered for each meeting is shown in parentheses: Congress of the Interamerican Society of Psychology, Rio de Janeiro, December 1958 (3); Congress of the History of Science, Barcelona, August–September 1959 (6); Conference of the International Union for Scientific Study of Population, Vienna, August–September 1959 (5); Congress of the International Sociological Association, Perugia, September 1959 (5); International Institute of Administrative Sciences, West Germany (6); Congress of the International Statistical Institute, 1959 (if held outside North America—10); International Congress of Historical Sciences, 1960 (36); Congress of the International Union of Scientific Psychology, 1960 (15).

Forms for application for travel grants will be supplied by the council on request. Applications for grants for the December 1958 Interamerican Psychological Congress will be due *15 October*, and grants for this meeting will be announced as soon as possible thereafter. Applications for all meetings to be held in the summer of 1959 will be due 1 December 1958, and grants will be announced 1 March 1959 or earlier.

Atmospheric Sciences

The National Science Foundation announced on 7 July the establishment of a Program for Atmospheric Sciences in the Division of Mathematical, Physical,

and Engineering Sciences. Earl G. Droessler joined the staff of the foundation as Program Director for Atmospheric Sciences. He was formerly with the Office of Science, Department of Defense.

The Atmospheric Sciences Program is being established by the foundation as a result of recommendations by the President's Committee on Weather Control and the Committee on Meteorology at the National Academy of Sciences–National Research Council.

The new program will deal with meteorology including not only the more conventional type of meteorological research, but also energy transfer processes between earth, sea, and air; turbulent flow of gaseous fluids; heat-exchange processes; upper-atmosphere studies; atmospheric chemistry; and general circulation problems of the atmosphere and oceans. Attention will also be given to the field of cloud physics, especially the physics of precipitation, where much basic research must be pursued before the possibility of controlling or modifying weather can be evaluated.

Droessler brings to his new assignment wide experience in administration and coordination of military scientific research and development, and in research in meteorology and glaciology.

News Briefs

The first of July marked the centennial of the first reading of Charles Darwin's paper on the evolution of the species. The occasion was marked at a meeting in London of the Linnaean Society held in honor of Darwin and Alfred Russell Wallace, who, working independently, had arrived at a theory which was so strikingly similar that the papers were read together. The attending group of biologists learned that the first reading was passed over by the president of the society, Thomas Bell, as a matter of no great importance. He was disappointed, Bell said, that 1858 had "not been marked by any of those striking discoveries which at once revolutionize, so to speak, the department of science on which they bear."

* * *

The Martin Company has opened a laboratory for basic research in Baltimore, Md. Patterned after the Institute for Advanced Study at Princeton, the company's Research Institute for Advanced Study was established "to observe phenomena of nature and to encourage, promote, and support investigations in search of underlying knowledge of these phenomena. To conduct theoretical and experimental studies, to discover the fundamental laws which affect them. . . ." Operating without security restrictions and without specific program direction by the parent company, the institute's staff of 35 scientists pursue fundamental studies which may or may not have eventual application. Photosynthesis, cosmic ray studies, Einstein's relativity theory, and nonlinear differential equations are some of the current and projected subjects of inquiry.

Proposed Legislation

Of the many bills introduced in Congress, some have a special relevance to science and education. A list of such bills recently introduced follows:

H Con Res 325. Authorize Joint Committee on Atomic Energy to print for its use 10,000 copies of the public hearings on the physical research program as it relates to atomic energy. Price (D-Ill.). House Administration.

S 3753. Provide that the Secretary of Interior shall develop and carry out an emergency program for eradication of starfish in Long Island Sound and adjacent waters. Bush (R-Conn.). Senate Interior and Insular Affairs.

HR 12710. Provide for research into problems of flight within and outside the earth's atmosphere. McDonough (R-Calif.). House Select Committee on Astronautics and Space Exploration.

S 3898. Authorize the establishment of the Indiana Dunes National Monument. Douglas (D-Ill.). Senate Interior and Insular Affairs.

S 3892. Stabilize production of copper, lead, zinc, acid-grade fluorspar, and tungsten from domestic mines by providing for stabilization payments to producers of ores or concentrates. Murray (D-Mont.). Senate Interior and Insular Affairs.

S 3900. Liberalize tariff laws for work of art and other exhibition material. Javits (R-N.Y.). Senate Finance.

S 3932. Provide for federal assistance for construction and expansion of public community junior colleges. Yarborough (D-Texas). House Labor and Public Welfare.

S 3946. Amend provisions of Public Health Service Act re grants for hospital construction to include institutions for care and treatment of mentally retarded. Yarborough (D-Texas), Proxmire (D-Wis.). Senate Labor and Public Welfare.

HR 12771. Restore (for 1 year) the duties on aluminum and aluminum products established in the Tariff Act of 1930. Baker (R-Tenn.). House Ways and Means.

HR 12821. Require the Surgeon General to undertake a special research program on cystic fibrosis. Broomfield (R-Mich.). House Interstate and Foreign Commerce.

Scientists in the News

JOHN I. YELLOTT, former assistant director of Stanford Research Institute and executive director of the Association for Applied Solar Energy in Phoenix, Ariz., has announced formation of the firm John Yellott Associates with headquarters at 901 W. El Caminito, Phoenix, Ariz. The new firm will render consulting services in mechanical engineering, with specialization in various aspects of power generation by steam and gas turbines, fuel utilization, energy conversion, and particularly in applications of solar energy to special problems in space heating and cooling.

BRUCE WALLACE, geneticist at the Biological Laboratory, Cold Spring Harbor, has been named associate professor in the department of plant breeding at Cornell University, effective 1 September. Wallace will continue his research in *Drosophila* genetics and teach genetics and experimental evolution.

HARRY J. FULLER, editor-in-chief of the *American Journal of Botany* and editor of the *Plant Science Bulletin* since its founding, has resigned from these posts because of ill health. HAROLD C. BOLD, professor of botany at the University of Texas, will succeed Fuller on the *American Journal of Botany*. HARRIET B. CREIGHTON of Wellesley College, has been elected editor of the *Plant Science Bulletin*. Bold has resigned as secretary of the Botanical Society, effective 1 September.

S. B. SELLS, chief of the department of medical psychology at the Air Force School of Aviation Medicine at Randolph Field, Texas, has been appointed professor of psychology at Texas Christian University.

To honor outstanding engineering achievement in the field of agriculture, the American Society of Agricultural Engineers has awarded the John Deere Gold Medal to WALLACE ASHBY, Beltsville, Md.; and two Cyrus Hall McCormick Gold Medals—one to THOMAS CARROLL, Toronto, Ont., Canada, and one to DENT PARRETT, St. Joseph, Mich.

O. BURR ROSS of Salina, Kan., general manager of the Gooch Feed Mill Company, has been named head of the department of animal science at the University of Illinois College of Agriculture. He succeeds L. E. CARD, who recently accepted an assignment as group leader of the University of Illinois agricultural staff working under the university's International Cooperation Administration contract in India.

MURRAY RABINOWITZ of the Rockefeller Institute for Medical Research has been appointed director of the central Cardiopulmonary Laboratory at the University of Chicago, effective in September. Rabinowitz has been working with Fritz Lipmann's biochemical research laboratory at the institute.

This year's honorary degree recipients include the following:

PETER P. ALEXANDER, chairman of the board of Metal Hydrides, Inc., from Northeastern University.

ERNEST W. BURGESS, professor emeritus of the University of Chicago, from Western Reserve University.

WILLIAM H. EISENMAN, former national secretary for the American Society for Metals, from Western Reserve University.

JOHN F. ENDERS, professor of bacteriology and immunology at Harvard Medical School, from Western Reserve University.

GEORGE H. HART, emeritus dean of the School of Veterinary Medicine, University of California, Davis, from the University of California, Berkeley.

HELEN S. HOGG, professor of astronomy at the University of Toronto, from Mount Holyoke College.

JAMES G. HORSFALL, director of the Connecticut Agricultural Experiment Station, from the University of Vermont.

TSUNG DAO LEE, professor of physics at Columbia University, from Princeton University.

ROBERT T. LEGGE, emeritus professor of hygiene, from the University of California, Berkeley.

GUSTAV J. MARTIN, vice president and director of research of the National Drug Company of Philadelphia, from Philadelphia College of Pharmacy and Science.

ISIDOR S. RAVDIN, John Rhea Barton professor of surgery of the School of Medicine of the University of Pennsylvania, from Philadelphia College of Pharmacy and Science.

J. E. WALLACE WALLIN, retired director, division of special education and mental hygiene, department of public instruction, Wilmington, Delaware, from Upsala College.

H. BRADFORD WASHBURN, JR., director of the Boston Museum of Science, from Northeastern University.

SEWALL WRIGHT, professor of zoology at the University of Wisconsin, from Western Reserve University.

CHIEN SHIUNG WU, associate professor of physics at Columbia University, from Princeton University.

CHEN NING YANG, professor of physics at the Institute for Advanced Study, Princeton, N.J., from Princeton University.

WALTER BAADE, for 27 years a member of the staff of the Mount Wilson and Palomar Observatories, has retired from the California Institute of Technology and the Carnegie Institution of Washington.

Baade is noted for his discovery that two entirely different types of stars (populations I and II) exist. This discovery led to his recalibration of Cepheid variable stars, distance indicators in the Andromeda nebula, and his correction in the cosmic distance scale. His work, which was reported at the International Astronomical Union meeting in September 1952, indicated that all objects beyond the Milky Way are about twice as far from the earth as had previously been supposed. Baade has also collaborated with Rudolph Minkowski in the identification of radio sources with optically observed objects, and in the physical interpretation of the nature of these sources.

Baade, a native of Schröttinghausen, Germany, studied at the Universities of Münster and Göttingen. He received his Ph.D. degree in 1919 at Göttingen and in that same year was appointed to the staff of the Hamburg Observatory, where he remained until he received his appointment to the Mount Wilson staff in 1931.

F. W. SPIERS, professor of physics at the University of Leeds, Leeds, England, will be in the United States and Canada from 21 July till the end of August. He will attend the International Congress of Radiation Research in Burlington, Vermont, 10–16 August, and will also visit Cambridge, Mass.; Ottawa; Toronto; Lemont, Ill.; Warrendale, Pa.; Oakridge, Tenn.; and Washington, D.C.

H. G. THORNTON, A. V. HILL, RUDOLPH PETERS, GEORGE TAYLOR, and D. C. MARTIN have been appointed by the Royal Society to attend the eighth General Assembly of the International Council of Scientific Unions, at the National Academy of Sciences, Washington, 2–6 October.

C. KINNEY HANCOCK, professor of chemistry at the Agricultural and Mechanical College of Texas, has received the 1958 award of $1000 from the Association of Former Students of Texas A. and M. This award is given in recognition of research performed in addition to duties as a teacher.

A bronze plaque commemorating 38 years of outstanding service to Cornell University by emeritus professor WALTER C. MUENSCHER was dedicated recently at the university. Muenscher is an internationally known specialist in weeds and poisonous plants.

C. D. SHANE, director of the University of California's Lick Observatory at Mount Hamilton, has retired from the directorship after 13 years of service. His successor is ALBERT E. WHITFORD, director of the Washburn Observatory at the University of Wisconsin.

JAMES A. OLIVER has been appointed director of the Bronx Zoo. He had been curator of reptiles at the zoo since 1 September 1951, and assistant director since 28 April of this year.

EDWIN H. ELLISON, professor of surgery at Ohio State University College of Medicine, has been appointed professor and director of the department of surgery at Marquette University School of Medicine.

Recent Deaths

CONDIT W. CUTLER, New York, N.Y.; 69; professor of clinical surgery at Columbia University, 1947–54; director of surgery at the Goldwater Memorial Hospital on Welfare Island, 1939–53; specialist in surgery of the hands and joints; author of *The Hand: Its Diseases and Disabilities*; 6 July.

GRAHAM L. DAVIS, Morehead City, N.C.; 65; director of the division of hospitals for the W. K. Kellogg Foundation; president of the American Hospital Association 1945–48; 4 July.

ROBERTO FRANCO, New York, N.Y.; 84; founder and first president of the University of Los Andes, Bogotá; specialist in research on tropical diseases in North Africa and Colombia; 5 July.

FELIX FRISCH, Trenton, N.J.; 78; chief of neurology at McKinley Hospital; performed research in epilepsy on the staff of the New Jersey Neuropathic Institute in Skillman, 1938–45; former chief of neurology at the Rothschild Hospital in Vienna, Austria; 5 July.

MANUEL HERSCHDORFER, South Orange, N.J.; 61; head of the department of mathematics of Seton Hall University; taught at Johns Hopkins University and at Amherst College; 6 July.

NELSON S. MAYO, Highland Park, Ill.; 91; former professor of veterinary medicine; retired export manager for Abbott Laboratories; former staff member at Kansas State College, Connecticut State College, and Virginia Polytechnic Institute; 5 July.

SAMUEL T. YUSTER, Los Angeles, Calif.; 54; professor of engineering at the University of California, Los Angeles; former chairman of the petroleum engineering department at Pennsylvania State University; performed research on the production of petroleum and on the reduction of smog by altering automobile exhausts; 3 July.

Book Reviews

One Language for the World and How To Achieve It. Mario Pei. Devin-Adair, New York, 1958. xvi + 291 pp. $5.

Albert Guérard's *A Short History of the International Language Movement* was published in 1922. It was the work of a master of English prose, a past master in the art of presenting a chaotic subject in smoothly surveyable form. It has long since gone out of print. The fast-moving events in the field of interlinguistics have long since made it go out of date as well. To state that Mario Pei's newest book satisfies completely the very urgent need for a new and up-to-date Guérard is high praise indeed. It is fully merited. Unfortunately, Pei's book tries to do even more.

Part I of *One Language for the World* presents a survey of the world's linguistic maze and the resulting problems of international communication. This makes fascinating reading. Here Pei is concerned with a theme which only a linguist who is simultaneously a sociologist can handle in a meaningful fashion. Pei's encyclopedic fund of information enables him to handle his job superbly.

Part II is an account of past and present developments and proposals concerned with the problem of Babel. How the polyglot impasse solved itself for the nonce as a matter of historical fact in numerous specific situations and how individuals and groups have worked to solve it by planned intervention are described vividly, urbanely, and fairly. As for the planned or guided solutions, one has the impression that Pei takes seriously only the approaches represented by Esperanto and Interlingua. This is entirely as it should be. Esperanto emerges as a dream which the faithful believe will come true. Interlingua appears as a tool effective today in the specialized applications for which it was designed. Yet, it must not be thought that Pei as a historian and as a reporter fails to do full justice to the multifarious complexities—the amateurish naïveté, the sound philological research, the antics of the lunatic fringe, the philosophical probing, the chiliastic idealism—which make up the scientific, prescientific, and pseudo-scientific phases of interlinguistics.

The colorful appeal of Pei's subject matter and his grand skill in giving it its dramatic due cannot conceal the fact that the linguist Pei refuses steadfastly to come to grips with the anthropological associations of his science. He speaks of language and society and has nothing to say about language and culture. Surely we have no right to expect that every student of language should be a dyed-in-the-wool general semanticist or metalinguist, but we do have a right to expect him to heed the problems whose insistent reality has been clear ever since Korzybski and Whorf first pointed them out. It is perhaps symptomatic that in his entire volume Pei finds no occasion at all to mention Korzybski, and about Whorf there is only the misleading (or downright erroneous) remark that he—along with Duns Scotus and Scaliger—dreamed about a universal grammar.

Pei's refusal to take seriously the suggestion that diverse linguistic patterns are specific to diverse patterns of thought and that both are related in a unique and functional manner to specific cultural courses of development serves him in good stead in the third and final part of his book. Here he presents his own ideas on how to achieve one language for the world.

It's all very much simpler than most of us thought. The universal language will be selected by a congress of authorized representatives of the world's governments. What language will be chosen does not matter as long as all agree to adopt it. It may be oriental or occidental, obscure or illustrious, planned or natural, Chinese or English or Finnish or Ojibway or Volapük or Interlingua. It will take the delegates little more than a week to select it. It will take a committee of linguists a year to retouch it and the governments of the world five years to train the necessary teachers. Immediately thereafter it will begin to be taught in all the world's kindergartens on a par with the children's native speech. The beneficiaries of this type of training will reach adulthood as full-blown bilinguists. And "long before the middle of the twenty-first century, the person unable to speak, understand, read, and write the universal world language will be far more rare than the illiterate is today."

There are many reasons why this plan cannot work. The most important one is doubtless that it will never be tried. And it will never be tried because too many of us have recognized as real the problems we associate with the names of Korzybski and Whorf.

It is to be hoped that there will be a revised edition of Pei's book, restricted to Parts I and II—even though there is reason to fear that the whole venture was embarked upon exclusively for the sake of Part III. Minus the part for the sake of which it was written, Pei's book may well become a classic, as Guérard's has.

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Chemistry of Carbon Compounds. A modern comprehensive treatise. vol. IV, part A, *Heterocyclic Compounds.* E. H. Rodd, Ed. Elsevier, New York, 1957 (order from Van Nostrand, Princeton, N.J.). xxvi + 807 pp. $28.

This volume of Rodd is the first of three which will deal with heterocyclic compounds. A remarkable amount of information has been condensed into this book, which describes the synthesis and properties of thiiran, aziridine, oxetan, thietan, azetidine, pyrrole, furan, thiophen, pyrazole, iminazole, oxazole, thiazole, triazole, tetrazole, pyridine, and their hydro derivatives. Benzo derivatives of these compounds (indole, carbazole, quinoline, acridine, and so forth) are also included. The more complex natural products, such as alkaloids and porphyrins, which contain these heterocyclic rings will be discussed in the later volumes. Three of the more esoteric heterocycles described are an oxaazacyclobutane (page 27), a derivative of diazacyclopropane (page 20), and 1-azatricyclo-[3,3,3,0]undecane bromide (page 137).

The literature has been well covered, and a random check of 200 references showed that 24 percent had their origin in the years 1950–57, 23 percent in 1940–49, and 21 percent in 1930–39. Typographical errors seem to be inevitable in a book of this size. Thirty-one were detected; the majority were missing or extra bonds in the otherwise excellent structural formulae. Structures IX, XII, XIII, and XV on page 241 are incorrect.

The various chapters (by T. S. Stevens, J. D. Loudon, E. Hoggarth, and N. Campbell) are uniformly written and maintain the high standard set by the previous volumes. Owners of volumes I to III will need no further recommendation.

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Proceedings of the Rehovoth Conference on Nucular Structure. Held at the Weizmann Institute of Science, Rehovoth, 8–14 Sept. 1957, under the auspices of the International Union of Pure and Applied Physics. H. J. Lipkin, Ed. North-Holland, Amsterdam; Interscience, New York, 1958. xvi + 614 pp. Illus. + plates. $12.50.

The rapid growth of the volume of publications makes it almost impossible even for the specialist to keep abreast of current scientific work. Large conferences, devoted to a reasonably narrow field, increasingly become the means of communication among physicists. The proceedings of such conferences are almost the only up-to-date record of the available results and of the current thinking in a field. The present volume, appearing only five months after the conference took place, is bound to become useful, both by refreshing the memory of the participants of the conference and also by serving as an orientation, and perhaps even as an introduction to the subject, for those who were not present. It shares with other similar volumes the characteristic of great readability which records of verbal proceedings have, and also a certain sketchiness, which is, unfortunately, quite unavoidable.

The volume contains about 125 contributions. Most of these are, naturally, short communications. However, there are more or less comprehensive summaries on all the principal subjects of discussion: the shell-model (Eden on the theoretical foundation and Kurath on some of the detailed results); the unified model (Mottelson and Peierls); group-theoretical methods (Racah and Flowers); electromagnetic transitions (Wilkinson); finite size of the nucleus (Rose); beta decay and parity (Konopinski on theory and Langer on measurements); extranuclear effects (Abragam on theory and Frauenfelder on measurements); instruments (Gerholm); and measurement of short life-times (Devons and S. G. Cohen). About two-thirds of all the contributions were theoretical in nature, about one-third experimental. The average length of the summary papers is perhaps a shade shorter than most readers would prefer: few of them extend to more than ten pages. Some of the short communications do not cover a page.

The Rehovoth Conference had its full share of announcements of new and important results; these give added zest to such gatherings and a significance beyond that of disseminating information. Apart from a discussion of the parity problem (by Lee and by Wu), there was a discussion of the calculation of binding energies based on the shell-model, by Talmi; Elliott's views of the unified model—which promise to grow much beyond their present importance —were reviewed by Flowers; Peierls spoke on his and Yoccoz' views on the same subject; and Bromley reviewed evidence for collective properties of light nuclei. Very probably other important ideas have been proposed, the significance of which I have failed to appreciate. There are also three interesting and detailed articles on specific subjects: one by Bergstrom on the nuclei in the Pb^{208} region, one by Zweifel on the *K*-capture phenomenon, and one by Steffen on the measurement of β-γ angular correlations.

It must have been an interesting and spirited conference, and the record of it does great credit to the editor. There was also some good-natured fun, recorded at the end of the volume. It should be noted that Pauli appears twice in the group picture of the participants but only once as contributor.

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Puzzle-Math. George Gamow and Marvin Stern. Viking, New York, 1958. 119 pp. Illus. $2.50.

This is a very enjoyable collection of 32 amusingly told little stories. Each story consists of two parts. The first part, printed in ordinary type, leads up to a neatly formulated puzzle; the second part, printed in italics, presents the solution of the puzzle. The ambitious reader should lay down the book when he has finished the ordinary type; he should try to find the solution by himself and, having found it, compare it with the italics. In doing so, he will learn more and have more fun than the lazy reader who reads the italics right away. Yet even the lazy reader may derive a lot of pleasure and profit from these little stories, because they are not pointless. There are whimsical details, some of which are emphasized by amusing drawings. Yet behind such details there is an essential mathematical principle in some stories, or an instructive method of solution or a side glance on physics in other stories. Still, the great majority of the puzzles are in all details accessible to the intelligent layman. Less than half a dozen solutions use a little high-school algebra, and in some of these cases the reader can skip the algebra and still understand the main point.

In such matters, it is difficult to be original. Three or four problems were new to me. I knew the rest, except certain details of presentation; of course, details of presentation are important here. I saw certain old acquaintances, with which friends used to tease me when I was an undergraduate, printed here the first time. And so the authors deserve praise also as collectors of mathematical folklore.

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Problema Protivorakovikh Antibiotikov. (Problem of Anticancerous Antibiotics.) N. G. Klueva and G. I. Roskin. Gosudarstvenoi Kontrolnoi Institut Sivorotok i Vaksin imeni L. A. Tarasevicha, Moscow, U.S.S.R., 1957. 247 pp. 10 rubles.

Shortly after World War II, considerable interest was aroused by the work of Nina Klueva and Gregory Roskin of Moscow, who reported that lysates of *Trypanosoma cruzi* (called "KR" from the initials of the authors) inhibited the growth of tumors in mice and had beneficial effects in some patients with cancer. Hauschka [*Cancer Research* **7**, 717 (1947)] and Belkin [*ibid*, **9**, 560 (1949)] could not duplicate the effects in mice, although their work included trypanosomes from the same source as those used is Moscow. Malisoff [*Science* **106**, 591 (1947)] claimed to have reproduced the findings, but his work was shown to be faulty.

Discussion of the KR preparation then became enmeshed in political complications and disappeared from the Soviet medical literature until 1956. This book summarizes the investigations and includes the findings in the use of the lysate in over 100 patients; of these cases 30 are reported in some detail. Unfortunately, the data raise more questions than they answer.

Daily intramuscular injections were administered for several months to 24 patients with carcinoma of the lip. The authors state that some effect was observed in 19 patients, and reports are presented on nine. Review of these suggests that these include three acceptable

cases of 5-year, and two acceptable cases of 3-year, survival without clinical evidence of cancer. The investigations included 73 patients with breast cancer, with some effect claimed in 40 cases; reports are detailed on ten, and of these at least four can be discarded as being questionable. These cases are particularly difficult to interpret, since most of these were early neoplasms, diagnosed by clinical examination, followed only by palpation after biopsy. Study of a few patients with easily measurable metastatic lesions would have been more informative.

Personal communications with other cancer investigators in the Soviet Union indicate that the Klueva-Roskin research has been a disappointment. It is noteworthy that their latest book lacks the imprimatur of the Academy of Medical Sciences U.S.S.R., or, indeed, of the official publishing house "Medgiz." Nevertheless, I am left with an uneasy feeling that the final chapters on the lysates of *Trypanosoma cruzi* remain to be written.

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The Future Supply of Oil and Gas. A study of the availability of crude oil, natural gas, and natural gas liquids in the United States in the period through 1975. Bruce C. Netschert. Published for Resources for the Future by Johns Hopkins University Press, Baltimore, Md., 1958. xi + 134 pp. $3.

Since the United States is vitally dependent upon a continuing supply of petroleum and natural gas, it has been heartening, during the last decade, to see our traditional complacency regarding the adequacy of these resources giving way to national concern over how soon the rates of production will begin their inevitable declines. The latest study concerned with this problem is the book *The Future Supply of Oil and Gas,* by Bruce C. Netschert of the staff of the nonprofit organization Resources for the Future, Inc.

This study, which presents no new data, consists first of a presentation and a review of most of the estimates and opinions published since 1950, all of which have been attributed to "experts" or to "recognized authorities." Then follow the estimates given by the author himself: (i) The total crude oil awaiting future recovery in the United States is of the order of 500 billion barrels, and the total future supply of natural gas is of the order of 1200 trillion cubic feet. (ii) The production "availability" of oil will be 6 billion barrels per year for the year 1975, that for gas will be 20 trillion cubic feet per year for the year 1980. (iii) The peaks of production "availability" for both oil and gas will occur later than the year 1975. (The meaning of the term *availability,* as used by the author, is obscure, but he seems to imply that we could produce the specified amounts if we wished to do so.)

According to my calculations, the author's "availability" figures for the years specified imply a minimum ultimate cumulative production of about 450 billion barrels for crude oil and about 1500 trillion cubic feet for natural gas. Since these figures are about three times, for crude oil, and almost twice, for natural gas, those cited only two years ago by Wallace E. Pratt [*Peaceful Uses of Atomic Energy* (U.S. Government Printing Office, Washington, D.C., 1956), vol. II, pp. 89–105] on the basis of replies to a questionnaire from 22 leaders of the petroleum industry, the critical reader will wish to know how so wide a discrepancy has arisen. To me it seems to have been the result of several complementary procedures.

In the first place, the author's weighting of "expert" opinions has been extraordinarily uncritical. The most flagrant example of this has been his inclusion in Tables 1, 2, and 4 of a completely "wild" figure of a reserve of 1000 to 2000 billion barrels of oil, given by a man having no firsthand knowledge or experience whatever in either the exploration for or the production of oil and gas, while excluding the estimate of 170 billion barrels of liquid hydrocarbons (which would include about 145 billion barrels of crude oil) obtained by Wallace E. Pratt as representing the ultimate reserves (both past and future production), figured on the basis of a recovery factor of 40 percent of the oil in place. The Pratt estimate implies the initial existence underground of a total of only about 360 billion barrels of crude oil.

The second procedure, which has been followed systematically, has consisted in choosing for each factor having a range of uncertainty that value which will tend to maximize the final results. Thus, an allusion has been made to the possibility of oil occurring to a depth of 65,000 feet, whereas the greatest depth drilled to date is about 22,500 feet. Reference has also been made to the possible occurrence of oil in the Precambrian. Similarly, to maximize the estimate of the amount of oil underground, Pratt's figure of 40 percent for the present average recovery factor has been rejected in favor of the lower figure of 33⅓ percent; whereas, according to George H. Fancher, the average recovery factor for Texas, which accounts for about 40 percent of the total United States production, is now about 46 percent.

By such procedures the author has arrived at estimates of reserves, and of future productive capabilities, for both oil and gas which, in the light of present information, crowd the upper limits of plausibility. These conclusions, accordingly, imply the existence for the next several decades of a state of national self-sufficiency with respect to petroleum and natural gas which may be more illusory than real. Consequently, should they be accepted at face value and made the basis for national policy, the results could prove to be detrimental to the national welfare.

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Structure Reports, 1951. vol. 15. A. J. C. Wilson, General Ed. International Union of Crystallography; Oosthoek's, Utrecht, Netherlands, 1957. viii + 588 pp. $29.

This volume maintains the high standard of clear and critical description which has made the *Structure Reports* series an indispensable reference source for chemists, crystallographers, metallurgists, mineralogists, solid-state physicists, and all who are concerned with the atomic structure of matter. Once again we should thank the editors and their reporters for their painstaking contribution to the problem of encompassing the accomplished work in this productive field of research.

Volume 15 is the first of those covering the second decade, since volumes 8 to 13 covered the period 1940–50, taking over from *Structurbericht,* which was discontinued after 1939, and volume 14 is the cumulative index, currently in preparation.

The work of *Structure Reports* was started in 1948, and volumes have appeared at the rate of one a year since 1951. As in nearly all branches of science, the amount of work to be reported has increased steadily. The editors are therefore faced with the dual problem of maintaining a steady output and bringing the series closer to date. To this end it is proposed to increase the number of section editors so that groups of abstractors can be working simultaneously on successive volumes. This is indeed welcome news, for the only limitation to the usefulness of the *Reports* has been the large gap between them and current research. A word of congratulation to the International Union of Crystallography is also in order for bringing about this most successful venture in international cooperation. Under

the new editorial arrangements the metals section will be reported by a group from the United States; the inorganic section, by a Dutch and a French group; and the organic section by a British and an American group, while the general editor is from Wales.

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COWA Survey and Bibliography. Current work in Old World archaeology and current publications in Old World archaeology. Lauriston Ward, Ed. Council for Old World Archaeology, Cambridge, Mass., 1957. $4.

With the announcement of a new service to provide archeological information for scholar and layman alike, the Council for Old World Archaeology, a newly formed organization, under the direction of its editor-in-chief, Lauriston Ward, and with the help of area editors, has issued its first *Bibliography and Survey*. This issue (the first of a biennial series) contains 133 pages of compressed information dealing with the areas of Central Europe, the Eastern Mediterranean, Northwest Africa, Western Asia, Northern Asia, and Indonesia. This eagerly awaited publication fulfills the highest expectations of the most critical student and meets his needs through the scholarship of its editors and their careful screening of the material. We are promised in the next offering (to be published shortly) the *Surveys and Bibliographies* for the British Isles, European Russia, West Africa, Southern Asia, and the Pacific Islands. Thus, during the current year, one half of the 22 areas of the Old World, as divided by the Council, will have been covered in this monumental and laborious project. This volume is an essential tool in archeology of the Old World, and it would be foolish economy not to include it on the reference shelf.

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New Books

The Systematics of North American Daphnia. vol. XIII. Memoirs of the Connecticut Acad. of Arts and Sciences. John Langdon Brooks. The Academy and Yale Univ. Press, New Haven, Conn., 1958. 180 pp. $8.

Person Perception and Interpersonal Behavior. Renato Tagiuri and Luigi Petrullo. Stanford Univ. Press, Stanford, Calif., 1958. 412 pp. $7.50.

Calculus of Variations and Its Applications. Proceedings of symposia in applied mathematics, vol. VIII. Lawrence M. Graves, Ed. McGraw-Hill, New York, 1958. 158 pp. $7.50.

Automatic Control, Principles and Practice. Werner G. Holzbock. Reinhold, New York; Chapman & Hall, London, 1958. 265 pp. $7.50.

The First Ten Years of the World Health Organization. World Health Organization, Geneva, Switzerland, 1958. 548 pp. $5.

Studies on Fossil Vertebrates. Presented to David Meredith Seares Watson. T. Stanley Westoll, Ed. University of London, Athlone Press, London; Essential Books, Fair Lawn, N.J., 1958. 275 pp. $5.60.

Technology of Instrumentation. Eric B. Pearson. Van Nostrand, Princeton, 1958. 202 pp. $4.75.

The Psychiatric Hospital as a Small Society. William Caudil. Harvard Univ. Press (for the Commonwealth Fund), Cambridge, Mass., 1958. 430 pp. $6.50.

Mathematical Tables and Formulae. F. J. Cramm. Philosophical Library, New York, ed. 6, 1958. 144 pp. $2.75.

Mathematics for the Layman. T. H. Ward Hill. Philosophical Library, New York, 1958. 343 pp. $4.75.

Matter, Earth, and Sky. George Gamow. Prentice-Hall, Englewood Cliffs, N.J., 1958. 606 pp. $10.

Population: an International Dilemma. A summary of the proceedings of the Conference Committee on Population Problems, 1956–1957. Frederick Osborn. Population Council, New York 17, 1958. 106 pp. $2.

An Introduction to Chemistry. Charles Compton. Van Nostrand, Princeton, N.J., 1958. 621 pp. $6.85.

Life Insurance and Medicine. The prognosis and underwriting of disease. Harry E. Ungerleider and Richard S. Gubner, Eds. Thomas, Springfield, Ill., 1958. 1012 pp. $16.50.

The Clinical Physiology of Physical Fitness and Rehabilitation. Ernst Jokl. Thomas, Springfield, Ill., 1958. 211 pp. $8.50.

The Institutions of Advanced Societies. Arnold M. Rose. University of Minnesota Press, Minneapolis, 1958. 703 pp. $10.50.

New Dimensions of Learning in a Free Society. Seminar addresses, discussions, public lectures, inaugural addresses delivered on the occasion of the inauguration of Edward Harold Litchfield, twelth chancellor, University of Pittsburgh, 9–11 May 1957. University of Pittsburgh Press, Pittsburgh, Pa., 1958. 298 pp.

Crime and Insanity. Richard W. Nice. Philosophical Library, New York, 1958. 287 pp. $6.

Physics and Mathematics. vol. 2. D. J. Hughes, J. E. Sanders, J. Horowitz, Eds. Pergamon Press, New York and London, 1958. 382 pp. $14.

Handbuch der Physik. vol. 45, *Nuclear Instrumentation, II.* S. Flügge, Ed. Springer, Berlin, 1958. 551 pp. DM. 128.

Annual Review of Plant Physiology. vol. 9. A. S. Crafts, Ed. Annual Reviews, Palo Alto, Calif., 1958. 520 pp. $7.

Processed Plant Protein Foodstuffs. Aaron M. Altschul, Ed. Academic Press, New York, 1958. 971 pp. $26.

Miscellaneous Publications

(Inquiries concerning these publications should be addressed, not to Science, *but to the publisher or agency sponsoring the publication.)*

Tarsal Ligaments of the Spectacled Bear. Tremarctos ornatus. Fieldiana: Zoology, vol. 39, No. 13. D. Dwight Davis. 14 pp. $0.40. *Notes on Fishes of the Genus Brachygobius.* Fieldiana: Zoology, vol. 39, No. 14. Robert F. Inger. 10 pp. $0.25. *Mammals of the Kelabit Plateau Northern Sarawak.* Fieldiana: Zoology, vol. 39, No. 15. D. Dwight Davis. 28 pp. $0.50. *Orchids of Peru.* Fieldiana: Botany, vol. 30, No. 1. Charles Schweinfurth. 260 pp. $4. Chicago Natural History Museum, Chicago, 1958.

Rowett Research Institute, Collected Papers. Preface; summary and subject reviews. vol. XIV. Reid Library, Rowett Research Inst., Aberdeenshire, England, 1958. 54 pp. Free.

Review of Medical and Veterinary Mycology. vol. 3, pt. 1, March 1958. Compiled from world literature on mycoses of man and animals. Commonwealth Mycological Inst., Kew, Surrey, 1958. 32 pp. 7s. 6d.

Improving College Biology Teaching. Subcommittee on College Education of the Committee on Educational Policies; Thomas S. Hall, chairman. Publ. 505. Division of Biology and Agriculture, National Academy of Sciences–National Research Council, Washington 25, 1957. 70 pp. $1.

The Pool and Irving Villages. A study of Hopewell occupation in the Illinois River Valley. John C. McGregor. Univ. of Illinois Press, Urbana, 1958. 232 pp. $3.50.

Experiments and Problems for College Chemistry. J. E. Belcher and J. C. Colbert. Appleton-Century-Crofts, New York, alternate ed. 5, 1958. 214 pp. $2.75.

Economic and Social Implications of Automation. A bibliographic review. Gloria Cheek. Labor and Industrial Relations Center, Michigan State Univ., East Lansing, 1958. 125 pp. $1.25.

Water for Industrial Use. United Nations, Department of Economic and Social Affairs, New York, 1958. 44 pp. $0.50.

The World Health Organization—Its Global Battle against Disease. Public Affairs Pamphlet No. 265. Albert Deutsch. Public Affairs Pamphlets, New York 16, 1958. 20 pp. $0.25.

Microscopic Staining Techniques, No. 4. Edward Gurr. Edward Gurr, Ltd., Michrome Laboratories, London, S.W.14, 1958. 66 pp. $1.

The Lynn Index. A bibliography of phytochemistry. Monogr. II. John W. Schermerhorn and Maynard W. Quimby, Eds. Massachusetts College of Pharmacy, Boston, 1958. 39 pp.

Symposium on Digital Computing in the Aircraft Industry. 31 Jan.–1 Feb. 1957. New York Univ., College of Engineering and International Business Machines Corp. 399 pp.

Properties and Numerical Relationships of the Common Elements and Compounds. J. E. Belcher and J. C. Colbert. Appleton-Century-Crofts, New York, alternate ed. 5, 1958. 366 pp. $3.25.

Reports

Common Antigens in Tissue Culture Cell Lines

Gey (*1*) has pointed out that pure lines of cancer cells and of normal cells from the same species can be grown in test tubes in identical media free from tissue juices and multiple cell types present in fresh tissue. In such a controlled environment, one would expect the peculiarities of the cancer cell to become readily apparent. While many differences have been described, critical analysis shows that none are unique to the cancer cell (*2*). This is hard to explain since cancer cells can cause death when they are injected into a suitably prepared animal, while fresh normal cells so injected promptly disappear (*3*). Apparently the animal can readily distinguish between normal and cancer cells.

We wish to report the results of antigenic analysis of various tissue-culture cell lines by means of the complement-fixation test. In these studies, experimental animals were injected with pure cultures of normal or malignant cells grown in tissue culture, and differences in the immunologic response were sought. The results of these tests indicated extensive sharing of common antigens by long-term tissue-culture cell lines even between cells from different species, and this is the subject of this report (*4*).

Cell lines were grown in synthetic medium 199 (*5*) to which 10 or 20 percent horse serum was added and were injected into 4- to 6-lb white New Zealand rabbits. Immune sera from these rabbits were reacted with cell antigens grown in medium 199 containing serum from a different species. A modified Kolmer technique was used with overnight fixation, two exact units of complement titrated in the presence of antigen, and two optimal units of antigen as determined by block titration with homologous antiserum.

As is shown in Table 1, similar antigens were found in many long-term tissue-culture cells. Antisera prepared against HeLa (human epidermoid carcinoma of the cervix), human embryo intestine (derived from normal embryo tissue), and cynomolgus monkey heart (derived from normal adult monkey heart) gave almost identical complement-fixation titers with all three antigens.

These same sera either did not react or gave titers eightfold less than against the homologous antigen when they were tested with antigens prepared from first-generation monkey kidney tissue culture or fresh monkey heart as shown in Table 1. Control antigens showed that the cross reactions were not due to traces of foreign serum or to the tissue-culture medium. No agglutinins for sheep red blood cells were found in the antisera. Furthermore, Forssmann antigen was ruled out as the common antigen by adsorption of the immune sera with sheep red blood cells. Adsorption of sera with group A, Rh-positive human cells reduced the titer but did not remove the common complement-fixing antibodies.

Additional complement-fixation studies indicate that other long-term tissue-culture cell lines share common antigens with the three mentioned above. These include Hep-2, a human epithelial carcinoma of the larynx; C-3, a cell line derived by Salk from a benign fibromyoma of man: and ERK, embryo rabbit kidney established in tissue culture by Westwood. Two additional long-term tissue-culture lines, human kidney (Chang) and human conjunctiva (Chang), are less closely related to this group of long-term tissue-culture cells. On the other hand, additional fresh primary tissue-culture cells reacted poorly with antisera against the long-term tissue-culture cells. These include tissue-culture cells from rabbit kidney and human heart, and rabbit kidney suspension. In this group also fall fresh human sympathicoblastoma suspension and the long-term tissue-culture mouse L cell (Earle).

The significance of these observations is given some support by the fact that three separate investigators have observed similar antigenic crossings between HeLa cells and one or more long-term tissue-culture lines from other species (*6*). Interpretation of our findings must be guarded because the antigenic crossings observed were with sera of homologous high titer against a long-term cell line, while antisera against fresh tissue-culture cells were of homologous lower titer. Repeated injection of fresh primary tissue-culture cells into several rabbits has not produced a single high-titer antiserum, in contrast to the ease with which high-titer antisera were produced by injection of long-term cell lines.

Tissues taken directly from the animal, or first-generation tissue-culture cells, do not contain in high concentration the common antigens found in long-term tissue-culture cells. This suggests that the common antigens have been acquired, accentuated, or changed in avidity during long-term cultivation in tissue culture. The fact that one fresh human cancer suspension (sympathicoblastoma) does not contain the common antigen suggests that it is not associated with malignancy per se, and the fact that Earle's L cell does not contain it suggests that it is possible to maintain long-term tissue cultures without their acquiring common antigenic components.

Several possible theories can be suggested to explain how long-term tissue cultures might acquire common antigens: (i) contamination of all antigenically related cell lines by a common tissue culture cell, for example, HeLa cells; (ii) loss of species specificity by the cell or increase of a component essential for the free living state upon prolonged multiplication in vitro; (iii) contamination with a common virus or bacterium; or (iv) possibly, errors in the test, or in its interpretation.

1) Contamination of all the antigenically related tissue cultures with a common tissue-culture cell is a possible explanation. Past experience in bacteriology and virology has demonstrated many instances in which accidental contamination of laboratory stocks has occurred, and this can obviously occur with tissue cultures. However, there is evidence that this is not the case since the antigenically related cell lines differ from each other in chromosome number and pattern and in tumor formation in treated rats (*3*). It is also possible that the cell lines are all contaminated with a common cell from the serum used in the culture medium. This too is unlikely for the reasons listed. It could be eliminated as a possible source of error in future studies by heat inactivation of all sera used in the feeding of tissue cultures.

2) Reversion of cell lines to a common primitive cell which has lost part of its species specificity through long

All technical papers are published in this section. Manuscripts should be typed double-spaced and be submitted in duplicate. In length, they should be limited to the equivalent of 1200 words; this includes the space occupied by illustrative or tabular material, references and notes, and the author(s)' name(s) and affiliation(s). Illustrative material should be limited to one table *or* one figure. All explanatory notes, including acknowledgments and authorization for publication, and literature references are to be numbered consecutively, keyed into the text proper, and placed at the end of the article under the heading "References and Notes." For fuller details see "Suggestions to Contributors" in *Science* 125, 16 (4 Jan. 1957).

Table 1. Complement-fixation titer of rabbit antisera with various tissue-culture cell antigens. TC, tissue culture.

Antigen* and dilution	TC passage	Antiserum†				
		HeLa	Human embryo intestine	Cynomolgus monkey heart	Rhesus monkey	
					Kidney	Heart
		200	37	63	1	0
HeLa (1:8)	6	128‡	512	1024	0	4
Human embryo intestine (1:4)	36	64	512	1024	16	32
Cynomolgus monkey heart (1:8)	73	128	512	1024	0	8
Rhesus monkey kidney (Und.)	1	16	8	32	32	4
Rhesus monkey heart (1:16)	0	4	0	8	0	32
		Controls				
Tissue culture medium and antibiotics		0	0	0	0	0
Horse serum (1:64)		0	0	0	0	0
Calf serum (1:4 to 1:1024)		0	0	0	0	0
Human serum (1:1024)		4	0	4	0	0
Monkey serum (1:1024)		0	0	0	4	4

* All cells for CF antigens were grown in calf serum and medium 199 and were standardized to contain 2.5×10^6 washed cells per milliliter and diluted as indicated to obtain two units of antigen in the dilution showing greatest complement fixation with its homologous antiserum as shown by a block titration.
† All reactions were carried out in the presence of two exact units of complement titered in the presence of antigen. Cells for preparation of antisera were grown in horse serum.
‡ Endpoints read from highest dilution tubes showing 3 + or 4 + fixation.

growth in tissue culture is an intriguing concept which would readily explain the observed partial or complete antigenic cross reactions as well as the morphologic similarities of long-term tissue-culture cells and possibly their relationship to malignant cells. That is, if long-term tissue-culture cells from various species gradually acquire a preponderance of a common antigen or antigens as they adapt to independent growth in vitro as well as the property of unrestrained growth in vivo when transplanted back to cortisone- and x-ray-treated rats (*7*), then a new laboratory experimental approach to study of malignancy may be at hand.

This last hypothesis can be approached in two ways: (i) by ruling out all other causes of cross antigenicity and (ii) by starting new cell lines from normal tissue under rigidly controlled conditions to prevent contamination and observing whether these new lines also revert to the common antigenic cell. Such studies are now under way.

3) The third and most likely explanation for common antigenicity of long-term tissue-culture lines derived originally from different animal species is contamination with a common virus or bacterium. To escape detection, such a contaminant would have to be resistant to the antibiotics in the culture medium and should not cause obvious turbidity of the medium, should fail to grow in routine bacteriological culture media, and should cause no extensive cytopathogenic changes in the tissue-culture cells. We have searched for bacterial and viral contaminants by the usual methods, but it is not possible to rule out the presence of an unknown noncytopathogenic virus or bacterium by means of negative tests. A slow-growing diphtheroid-like organism in one of our tissue-culture lines did not share common antigens with the long-term cell lines. Pleuropneumonia-like organisms (PPLO) have been reported in tissue-culture cell lines by Robinson, Wichelhousen, and Raizman (*8*) and are present in our HeLa cell lines. Extensive search by Morton and Rothblat (*9*) has revealed PPLO in several but not all long-term cell lines maintained in this laboratory and in cell lines from other laboratories. CF tests with the PPLO antigen, T5, isolated from our HeLa culture and antisera against the long-term tissue-culture cells were all positive. Similar tests with a PPLO antigen prepared from a laboratory strain of human genital origin, O7, were all negative. These tests suggested that the contaminating PPLO may be one common antigen shared by some long-term cell lines. However, adsorption of the long-term tissue culture antisera with PPLO-T5 did not remove the common antibody for long-term tissue-culture antigens. This, combined with the fact that some of the cell lines did not contain PPLO, suggests that these contaminating organisms (PPLO) are not the explanation of the common antigens in long-term cell lines. However, cell lines may contain different antigenic types of PPLO, and this has not been investigated.

These studies raise more questions than they answer, but two points seem to be fairly well established: (i) The acquisition of common antigens by long-term tissue-culture lines occurs frequently and crosses species lines. The explanation for this is not clear although several possibilities are offered. (ii) Modern tissue-culture media plus antibiotics provide an ideal medium for PPLO, viruses, and some fastidious bacteria and their presence may not be revealed by routine bacteriological sterility tests (*10*).

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12 February 1958

Phosphorescence in Liquid Scintillation Counting of Proteins

Alcoholic solutions of the quaternary base Hyamine have been reported by Vaughan *et al.* (*1*) as being suitable solvents for amino acids and proteins in liquid scintillation counting systems. While investigating this Hyamine system for the counting of proteins and tissues, we encountered, when working with larger weights of such substances, a phosphorescence phenomenon, which, if it is not eliminated, can cause considerable trouble in the counting of samples of this magnitude. Fortunately methods are available whereby such interference can be eliminated or circumvented.

The substances used were dried rat bile, rat liver, and horse serum; whole rat muscle; egg albumin, gelatin, Bactopeptone, and trypsin. As much as 100 mg, and in some cases more, of these substances can be dissolved in 3.0 ml of

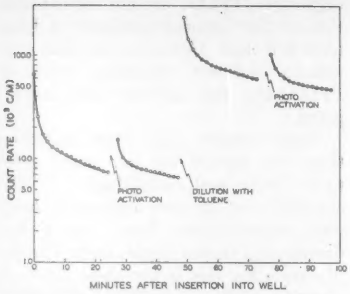

Fig. 1. Count rate of a trypsin-Hyamine solution showing the effects of exposure to light and of dilution with toluene.

Hyamine solution, prepared according to the method of Vaughan (*1*). These 100-mg protein samples usually required heating between 50° and 70° C for periods ranging from ½ hour to several hours. Samples were then cooled to room temperature, diluted with 10 ml of toluene containing 0.5 percent DPO, cooled, and counted. All counting was done with a Tri-Carb liquid scintillation counter operated at 1100 v with channel settings 6–35 and 6–100.

It was observed that neither radioactive isotope nor scintillation phosphor was needed to obtain a tremendous count rate. From 20,000 to 300,000 count/min was observed with Hyamine-protein-toluene solutions of 100-mg portions of the proteins mentioned above. This count rate decreased regularly but slowly. The normal instrumental background was reached only after several days. (For rate studies zero time was taken as start of addition of toluene to the protein-Hyamine solution.) It was also observed that protein-Hyamine solutions containing no added toluene (or radioisotope) exhibited this slow-decay phosphorescence. (For these solutions, zero time was taken as time of insertion of the cooled sample into the counting well of the counter.) In general, the magnitude of the count was not as great (up to 50,000 count/min) as it was when toluene was present. When these protein-Hyamine solutions were diluted with toluene, alcohol, acetone, dioxane, ether, water, or glycerine, the rate was raised; the first five substances yielded a count rate of several hundred thousand counts per minute. The count rate on dilution seemed to be an inverse function of the viscosity of the diluting solvent; the lower the viscosity, the higher the rate.

The count rate of both the protein-Hyamine and protein-Hyamine-solvent solutions could be increased by exposure of the solutions to an incandescent lamp. The entire phenomenon is shown in Fig. 1 for 100 mg of trypsin dissolved in 3.0 ml of Hyamine solution. Here are shown the initial count, a photoactivated count, the count after dilution with 10 ml of toluene, and another activated count. The activation consisted of exposing the sample to the light of a 75-w Reflectorspot at 25 cm for 1 minute. (Empty vials, or vials containing toluene, gave no more than a 2-count/min increase when they were so illuminated for 5 minutes.)

In all cases, the initial count increased as sample weight increased. Below a concentration of about 10 mg of protein per milliliter of Hyamine solution, the phenomenon was negligible so that the phenomenon described does not invalidate the procedure of Vaughan *et al.* for either C^{14} or H^3 counting. In several of the experiments when the protein-Hyamine solution was diluted with toluene or other solvent, the initial rate started at zero and increased within a few seconds to a maximum and decayed in the usual way. In all experiments, the difference in count rate between the two channels soon became constant and small, showing the pulse heights of the counts to be chiefly below the 35 setting. The variation of the rate with photomultiplier voltage was as follows (calling the rate at 1100 v 1.00): 1020, 0.26; 1180, 1.35; 1260, 1.28; 1340, 0.88; 1400, 0.46.

Debye and Edwards showed that proteins in alkaline solution exhibit phosphorescence (*2*). They attributed this chiefly to tyrosine and tryptophan. As a check, 100-mg portions of these two amino acids were allowed to stand with 3.0 ml of Hyamine solution until solution occurred and were then counted after addition of toluene. Both of the two gave a slight, fairly rapidly decreasing count rate: tyrosine, 1500 count/min; tryptophan, 3500 count/min. In about 10 minutes the normal background was obtained. Exposure of the solutions to the Reflectorspot gave no increase in count rate for the tyrosine; the tryptophan rate increased by 300 count/min.

Another sample of trypsin, a substance which in 100-mg portions in Hyamine solution gave a tremendous count rate, was dissolved in 1.0*M* KOH and counted. No count was obtained. Hyamine solutions without protein gave no count either alone or when diluted with toluene or when exposed to light. Hyamine solutions of sterols, or of amino acids other than those mentioned above, failed to provide counts. For a given Hyamine preparation there seemed to be a possible correlation between initial count rate and number of hydrolyzable bonds. The count rate for equal weights of glycine, glycylglycine, glutathione, Bactopeptone, and trypsin increased in the order given.

The precise cause of this phosphorescence is not known. Heating of the sample may be significant. However, the effect can be completely eliminated in several ways. As already mentioned, for small quantities of proteins the effect is inconsequential. The purity of the Hyamine itself is of prime importance. Repeated crystallization of the original quaternary chloride from toluene, until the filtrate is colorless, yields Hyamine which, when it is converted to the quaternary base in methanol solution, with small weights of nonradioactive protein and several hours of sample cooling, produces only normal instrumental background. Large weights of proteins can be used if the solution is acidified prior to counting. Acidification of a high-counting protein-Hyamine-toluene solution reduced the count rate immediately to normal instrumental background. Such acidified solutions can be counted immediately after acidification. Little or no change in counting efficiency results from acidification.

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4 April 1958

Isometric Twitch Tension of Frog Skeletal Muscle as a Function of Temperature

Current work at this laboratory on the action of high-intensity ultrasound on the twitch contraction of excised frog skeletal muscle makes it necessary to distinguish between temperature and nontemperature effects produced by the sound. Therefore a study (*1*) was undertaken to determine the variation in the magnitude of the maximum isometric tension and the action potential of excised frog biceps muscle at a number of temperatures in the range 2° to 35°C. Unfortunately, in the recent literature there appears little data on frog skeletal muscle twitch tension at temperatures above 25°C. Walker (*2*), working at the two isolated temperatures 14.5° and 27.5°C found different results for *in situ* summer and winter frogs. His experiments on excised muscles produced variable results, with an indication that twitch tension is less at the lower temperature. Hill's well-known results (*3*) are restricted to temperatures below 22°C; Buchthal's results (*4*) on single fibers are limited to temperatures up to 26°C. The work of early investigators, referenced later in this report, yielded variable results.

The following procedure was used in a series of 32 experiments. At an initial

temperature of approximately 20°C, the muscle was stimulated once every 10 seconds for a period of 2 minutes, while the action potential and isometric tension were recorded. The Ringer's solution was then siphoned from the bath container, and a solution at a new temperature was added. After an acclimatization period of 3 minutes at this new temperature, the muscle tension and action potential were again recorded. This procedure was repeated at a number of different temperatures, after which the muscle was returned to the initial temperature of 20°C to check reversibility. All muscles were tested at the experimentally determined "rest length" position. Both curarized and noncurarized preparations were studied. An RCA 5734 transducer tube, in conjunction with an amplifier and recorder, was used to measure the muscle tension. Isometric operating conditions were insured by the use of a double-spring system in association with the transducer tube shaft. A copper constantan thermocouple, inserted through the center of some of the muscles, indicated the internal temperature. The action potential was recorded by photographing the trace of an oscilloscope screen.

Electrodes, consisting of silver-plated Nichrome pins inserted completely through the muscle, were used for both stimulation and for picking up electrical responses. These electrodes, originally intended for the ultrasound experiments, were designed as follows. The end of each pin, which is fastened to a flexible lead, is covered by a plastic fitting against which the muscle rests snugly when light pressure is applied by a plastic nut which screws on the free end of a threaded pin after the pin is inserted through the muscle. This design eliminates relative motion between the muscle and each electrode, despite gross movements of the muscle. In the majority of the experiments, wherein both the mechanical and electrical responses were recorded, the stimulating electrodes consisted of a single pair of closely spaced pins rather than a series extending throughout the length of the muscle. In such experiments, the stimulus level used was one yielding maximum amplitude of action potential but not necessarily maximum isometric tension. The results obtained on the mechanical response with such electrodes were checked by a number of experiments with stimulation accomplished by a series of electrodes with an average spacing of 1½ mm along the entire length of the muscle. When this electrode arrangement was used, the stimulus level was graded, at each temperature, from a value just above threshold to one well above that yielding maximum isometric tension.

For temperatures below 20°C, the results of the experiments reported here on maximum isometric twitch tension agree with those found in the literature—that is, as the temperature decreases the amplitude of the tension increases, while the speed of contraction or shortening decreases. However, for temperatures above 20°C the amplitude of the isometric tension again increases, while the speed of contraction and relaxation also increases. The relation between the amplitude of maximum tension and the temperature is indicated in Fig. 1. The changes with temperature, over the range indicated on the graph, are reversible. Similar results were found on both curarized and noncurarized preparations. No indications of heat contractures were obtained at temperatures up to 35°C. Over approximately the same temperature range the isometric *tetanic* tension is, according to A. V. Hill (*5*), a monotonic function of the temperature, rising relatively slowly. [Hajdu's reported results on tetanic tension (*6*) exhibit a maximum within this same temperature range.]

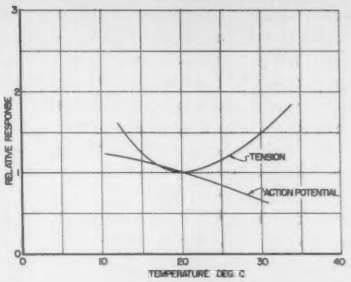

Fig. 1. Amplitude of maximum isometric twitch tension and action potential (macroelectrodes) as a function of temperature for the excised frog biceps muscle.

The electrical measurements show that the amplitude of the muscle action potential, when macroelectrodes are used, decreases monotonically with increasing temperature over the range 2.0° to 32°C. In addition, the "expected" increase in conduction velocity and decrease in duration of electrical response occur with rising temperature. These results agree with those of Sanderson (*7*), who worked with frog sartorius in the range 4° to 20°C, and with those of Welkowitz and Fry (*8*), who worked with frog biceps muscle, in the temperature range 20° to 35°C. The increase in electrical conduction velocity with increasing temperature, determined from our experiments on whole muscle, agrees very closely with the results of Wilska (*9*), who found a factor of 5.4 for the increase in velocity, for single fibers of frog skeletal muscle, as the temperature is raised from 0° to 36°C.

The fact that the action potential versus temperature relation follows a monotonically decreasing curve with rising temperature, while the amplitude of the isometric tension versus temperature goes through a minimum, is of interest, but it does *not* prove that the action potential is unnecessary for initiation of the contractile process. The amplitude of the action potential over the entire temperature range studied may always be much greater than the minimum electrical stimulus required to initiate a mechanical response.

The isometric tension curve of Fig. 1 is of interest in regard to present theories on the variation of the amplitude of twitch tension with temperature. The problem, as formulated up to the present time, has been to explain why the isometric tension decreases with increasing temperature, the highest temperatures generally considered being not much over 20°C. The explanation usually given is that at the higher temperatures the relaxation process overcomes the contraction process before full tension can be reached (*3, 10*). Since the duration of the active state is approximately halved for each 10°C rise in temperature, there is insufficient time for completion of internal shortening before decay of the active state (relaxation) begins (*11*). Theories based on such a hypothesis are inadequate for explaining the increased tension, at temperatures above 20°C, found in the authors' experiments.

Gad and Heymans (*12*), in 1890 reported results similar to those reported here on the variation of maximum twitch tension of excised frog skeletal muscle over the temperature range 0° to 30°C [see also Kaiser 1896 (*13*), Brodie 1898 (*14*), Carvallo and Weiss 1900 (*15*), de Boer 1915 (*16*)]. Unfortunately, Bernstein (*17*) attributed Gad and Heymans' results on increased tension at temperatures above 20°C to a threshold effect. The results of the experiments described here, in which multiple electrodes and supramaximal stimulation were used, definitely indicate that the increased isometric twitch tension at high temperatures is not a threshold effect. Work is now under way on a study of the form (tension as a function of time) of the isometric twitch response at temperatures above 20°C, in order to provide additional information on the behavior of isometric tension with temperature.

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28 February 1958

Deoxyribonucleic Acid Synthesizing Cells in Peripheral Blood of Normal Human Beings

Neoplasia, aplasia, and metaplasia are problems of great importance in medicine today. The mechanism of development of diseases involving these processes is unknown, and treatment is unsatisfactory. Perhaps this is not surprising since the mechanism of control of normal cell destruction and renewal is also obscure. The development of tritium-labeled thymidine, a specific deoxyriboside precursor of deoxyribonucleic acid (DNA), by W. L. Hughes (*1*) provided a powerful tool for the in vivo and in vitro study of cell turnover in man and animals. Individual cell histories can be followed easily using histoautoradiographic techniques (*2*). A review of earlier studies on protection against radiation-induced aplasia of the marrow (*3*) led to the hypothesis that totipotential primitive mesenchymal cells may be circulating under normal conditions. If this were in fact true, one might expect some of these cells to be actively synthesizing DNA preparatory to later mitosis. The in vitro studies reported here were designed to investigate this hypothesis (*4*).

Six healthy males 28 to 45 years of age with peripheral blood counts within normal limits were selected. Nine milliliters of blood from the antecubital vein were withdrawn under sterile conditions and placed immediately into 2 ml of a solution of normal saline containing 5 percent dextran, 2 mg of heparin, and tritiated thymidine. Dextran was used to accelerate erythrocyte sedimentation and to allow easy concentration of white cells (*5*). Tritium-labeled thymidine with a specific activity of 390 mc/mmole was added such that the final dilution was 2 μc/ml. The blood was allowed to incubate for 1 hour at room temperature, during which time the erythrocytes settled. The supernatant was withdrawn and centrifuged at 200*g* for 10 minutes. The cells were then resuspended to a concentration of 100,000/mm^3, and smears were made. The smears were fixed in absolute methyl alcohol, and stripping film was placed over the preparations as described by Pelc (*6*). The film was developed, and the autoradiographs were read after 15 days' exposure. Some smears from each sample were stained with Feulgen's stain prior to application of the photographic film; others were stained through the stripping film with Wright's stain after the film had been developed. Five thousand white cells of each individual were enumerated to determine the percentage of labeled cells. A 500-cell differential was done on each individual.

The results are summarized in Table 1. Labeled cells were found in the blood cell concentrate of all individuals studied. Between 10 and 60 grains were counted above each labeled cell. In terms of the total white cell count of the concentrate, the number of labeled cells amounted to 0.06 percent or less. A typical labeled cell is shown in Fig. 1.

Table 1. Labeled cells in white cell concentrates from the blood of normal human beings.

Subject No.	Differential counts of cell concentrates (%)				Labeled cells	
	Segmented leukocytes	Lymphocytes		Monocytes	Percentage of total cells	Percentage of large mononuclear cells
		Large	Small			
1	62	7	24	7	0.06	0.4
2	78	3	16	3	0.04	0.7
3	68	11	18	3	0.02	0.1
4	52	25	21	2	0.06	0.2
5	56	13	29	2	0.06	0.4
6	67	14	16	3	0.06	0.4

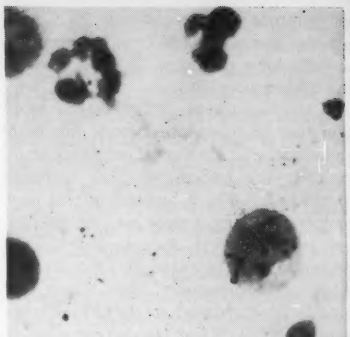

Fig. 1. Large mononuclear cell labeled with tritiated thymidine, found in the peripheral blood of a normal human being.

Labeling was seen only in large mononuclear cells identified as monocytes, or large- and medium-sized lymphocytes. No labeled cells of the myeloid series and no labeled typical small lymphocytes were found.

The existence in the peripheral blood of man of a small percentage (but large absolute number) of circulating leukocytes that have the capacity to synthesize new DNA has been demonstrated. The synthesis of DNA is presumptive but not direct evidence that the cell is destined to divide if given proper conditions for seeding, since present beliefs are that intracellular DNA is synthesized only in preparation for cell division. We consider these cells to be predominantly the classical monocyte. Some resembled large lymphocytes. The monocyte is a phagocytic cell. We, among others, have long considered this cell to be a tissue histiocyte in transit. Does the capacity to synthesize DNA and presumably divide indicate that this cell is in reality a totipotential primitive mesenchymal cell, or does it merely indicate that this cell has the capacity to propagate itself? This fundamentally important question has not yet been answered by direct experiment. The present studies neither support nor refute the contention of Bloom (*7*), Farr (*8*), and Yoffey (*9*) that lymphocytes may be totipotential cells that are in continual transport to the marrow and become transformed into other blood cell lines. Initial in vivo studies with human beings, to whom tritium-labeled thymidine was administered, resulted in the labeling of essentially no small lymphocytes in the peripheral blood, although a small number of labeled large mononuclear cells were found (*10*).

It is clear from current studies of patients with blood dyscrasias that, with

the same techniques reported here, it is possible for large numbers of cells in the peripheral blood to take up tritium-labeled thymidine. With the normal individuals, increase of the incubation time with tritium-labeled thymidine up to 24 hours did not increase the percentage of labeled cells. It thus appears reasonable that in the normal individual only a very small percentage of the cells of the peripheral blood are synthesizing DNA, and these presumably are capable of division. However, the average human being of 60 kg with 7 percent blood volume and 7000 leukocytes/mm³ has approximately $(0.06 \times 7000 \times 4200 \times 1000)/100$, or approximately 18×10^6 potentially dividing cells in the peripheral blood at one time and presumed to be in transit.

The findings that histiocytes, or specific cells of the reticuloendothelial system, may be multipotential in character, and may be transported to needed sites normally via the blood stream, is pertinent in connection with protection against X-radiation by parabiosis (*11*) and by regionally fractionated exposures (*12*). In the later experiments, exposure of one-half of the body only was followed in a few minutes by exposure of only the other half of the body. Mortality was less than it was in animals that received equivalent single-dose total-body irradiation. Normally circulating multipotential cells would explain these findings. The hypothesis is supported by estimates that only a very few intact cells are required to repopulate a radiation-depleted marrow population (*13*).

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26 February 1958

Two Syndromes Caused by Sweetpotato Viruses

Unpublished studies on the nature, host range, and seasonal development of the sweetpotato virus complex have distinguished two distinct virus syndromes, those of feathery mottle and internal cork.

In insect transmission experiments begun in 1954, of which a preliminary report was published (*1*), the aphids *Myzus persicae* (Sulz.) and *Macrosiphum solanifolil* (Ashm.) transmitted a virus from sweetpotato to sweetpotato that was distinguished by chlorotic spots on the foliage, which are typical of the internal cork syndrome. The aphids picked up the virus from two sources: (i) plants of the Porto Rico variety showing the internal cork virosis and (ii) plants of a Russian sweetpotato (plant introduction No. 105945) showing the feathery mottle virosis.

On the basis of the "flush of growth" indexing technique (*2*), which requires about 2 months, there was 100 percent transmission of the chlorotic spot symptom of the internal cork syndrome. Since the syndrome caused by the feathery mottle virus did not appear in plants inoculated with aphids fed on plants of the Russian sweetpotato, it appeared that the aphid was not the vector of the virus.

On the basis of the necrotic root symptom (*1*) of the internal cork syndrome (after storage of roots for 6 months at temperature of 75°F and at 90 percent relative humidity) there was approximately 20-percent transmission, both by insects and by grafting, under greenhouse conditions. The very mild root-symptom expression and the low percentage of incidence must be balanced against the fact that 100 percent of the roots produced some sprouts showing the same virus syndrome. It appears that the development of root symptoms requires a longer incubation period, or multiple inoculations such as undoubtedly occur in the field.

Further studies revealed that there is a definite but limited amount of natural transmission of the feathery mottle virus in the field. Such transmission is revealed only after harvest, in the sprout growth of the bedded, infected roots. The 1957 season was unusual because it stimulated an extended vegetative growth throughout September, after the growth had come to a halt during the summer drought in August, and there was positive evidence of current-season transmission of feathery mottle virus (Fig. 1*B*). Moreover, when the "flush of growth" technique (*2*) was used in the greenhouse with material harvested from the field, there was a much higher incidence of infection than is normally observed. The current-season symptoms, which appeared only on the terminal growth, were similar to those reported by Sheffield (*3*) and could be manifestations of the same disease.

A very important feature of the earlier transmission experiments involving internal cork was the discovery that the original source plant of feathery mottle virus (Russian sweetpotato, P.I. 105945) also contained the internal cork virus, or viruses, and that only the cork virus was transmitted by aphids. This feathery mottle source plant, which descended in a clonal line from plants used by Doolittle and Harter (*4*) in their work on feathery mottle and which was listed by them as No. 029878, also exhibited typical chlorotic spot of the internal cork virus syndrome, and this symptom has been constantly present in this seedling in subsequent studies.

That sweetpotato P.I. 105945 carried both viruses may possibly account for the fact that Webb (*5*) and Webb and Larson (*6*) were able to obtain chlorotic spotting in plants inoculated with virus from their feathery mottle source plants by means of the aphid vector and also by mechanical means. Even though sweetpotato P.I. 105945 has been used in numerous experiments, it has not yet exhibited the necrotic, internal cork root symptom, perhaps because of its immunity from expression of the necrotic root spotting phase of this syndrome.

During a sweetpotato virus disease sur-

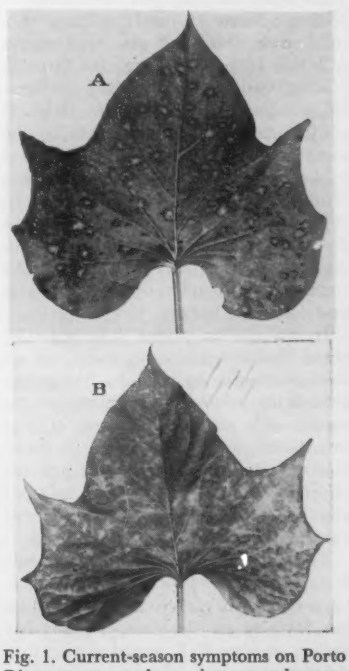

Fig. 1. Current-season symptoms on Porto Rico sweetpotato leaves in two syndromes: (*A*) internal cork; (*B*) feathery mottle.

vey in 1955–57, made in about 20 states, the syndromes here described were encountered side by side in the field in three states: in Georgia in 1955 and in California and New Jersey (7) in 1957. In Georgia the Porto Rico variety and several seedlings were observed to be infected with feathery mottle. In California, feathery mottle was found on Red Velvet, a pigmented selection from Porto Rico. In New Jersey the affected variety was Georgia Red. In all cases in other states the "yellow dwarf" symptom description of common use in California was found to be generally applicable.

Incidentally, two similar sweetpotato disease syndromes caused by viruses are known in other countries. The Russian sweetpotato P.I. 105945 exhibits both syndromes. Sweetpotato root material from Israel exhibited both the cork and the feathery mottle syndromes and, when indexed, was apparently identical with the United States material.

Sheffield (8) reported that at least two viruses, designated A and B, attack sweetpotatoes in East Africa. Virus A, found only in one locality, caused a relatively mild disease and was transmitted by *Myzus persicae*. Virus B was widespread throughout East Africa and was transmitted by a white fly (*Bemisia tabaci* Genn.). There are a number of strains of virus B, the mildest of which may cause no symptoms in some sweetpotato varieties, whereas the others cause extremely severe diseases. Infection with virus A did not protect a plant from infection by virus B, but infection with a mild strain of virus B protected against infection by a severe strain of virus B. Neither virus was transmitted mechanically to healthy sweetpotatoes. Sheffield conjectured that virus A might be the same as one of the sweetpotato viruses in the United States but considered virus B to be distinguished from them by its insect vector, the white fly. However, we do have an as yet unidentified insect vector for the feathery mottle American counterpart, to judge by the spread of this disease in nature.

In an earlier report Sheffield (9) noted that in some cases there was no obvious stunting by virus B, "the symptoms being confined to mottling, or one runner only may be affected or one branch only of one runner. It is probable that the stunted forms are primary infections (i.e., they arose from infected vines), the milder forms being secondary (or new) infections."

The appearance of similar current-season symptoms at Beltsville, Maryland, in 1957 in the syndrome caused by our feathery mottle virus confirms her finding on virus B. It would appear that differences in the climate or environment of the continents and the effect of these differences on the agents of dissemination might account for the apparent discrepancies in the disease syndromes caused by similar viruses, but the possibility of specific differences exists. The latter view is supported in the recent report by Sheffield (3), who describes the transmission of sweetpotato virus to other hosts (several species of *Ipomoea* and of the family Solanaceae) by mechanical inoculation, by graft, and by means of white flies.

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26 February 1958

Thorium Content of Ocean Water

Two samples of ocean water from the Pacific, one collected at the surface, near the coast, outside San Diego Bay in the summer of 1956, and the other taken at a depth of 3500 m from longitude 124° 41.0′ W, latitude 33° 54.5′ N on 25 March 1957, have been analyzed for total thorium alpha activity.

Each sample was approximately 25 liters in volume. Three hundred milliliters of concentrated HCl, 25 ml of 0.18*M* $Fe(NO_3)_3$ solution, and approximately 5000 count/min of Th^{234} beta tracer were added to each sample at the time of collection. The samples (in polyethylene containers) were stored for several weeks before they were analyzed.

The method of analysis was as follows. To carry the thorium, ferric hydroxide was precipitated by adding slight excess of ammonium hydroxide to the sample solution. The hydroxide precipitate was dissolved in dilute hydrochloric acid. Nitric acid and perchloric acid were added to the solution, which was then evaporated to fumes of perchloric acid. Addition of hydrofluoric acid and repeated fuming served to remove silica. The final solution was diluted to decrease the concentration of perchloric acid to 3*N* and was then transferred to a 4- by 100-mm Dowex-50 resin column to separate the thorium from carrier and extraneous ions (1). The thorium on the column was washed repeatedly with 3*N* hydrochloric acid. It was then eluted from the column with 0.5*M* oxalic acid. This eluant was treated with nitric acid and perchloric acid, heated to fumes of perchloric acid three times, diluted, and again transferred to the resin column. After washing with 3*N* hydrochloric acid, the carrier free thorium was eluted with 0.5*M* oxalic acid, and the eluant was collected on a tantalum plate. The plate was taken to dryness under a heat lamp, flamed, and counted for alpha and beta activity. The beta count served to determine the fraction of added Th^{234} which was recovered. From the observed alpha activity the total thorium alpha activity in the sample was calculated assuming complete exchange of thorium with the Th^{234} tracer.

In each of the samples analyzed, additional thorium was obtained from the first eluant and hydrochloric acid washings by taking these through further perchloric acid fumings and resin-column treatments. These second fractions of thorium which were separated from the surface water and deep ocean samples are designated below as S-2 and D-2, respectively.

Samples were counted on an alpha proportional counter at 51-percent geometry. The observed activities, corrected for yield, geometry, background, and reagent blank, are listed in Table 1.

The half-life values of Th^{232}, Th^{230}, Th^{228}, and Th^{227} (the only thorium alpha emitters which occur naturally) are 1.39×10^{10} years, 80,000 years, 1.90 years, and 18.6 days, respectively. Using these values, we calculate the following upper limits of concentration of thorium isotopes in the deep ocean sample (2): Th^{232}, less than 5×10^{-11} g/ml; Th^{230}, less than 3×10^{-16} g/ml; Th^{228}, less than 7×10^{-21} g/ml; Th^{227}, less than 2×10^{-22} g/ml.

Some information concerning the approximate isotopic composition of the thorium which was isolated from our surface water sample was obtained by observing the change in its alpha activity with time. The data for sample S-2 are shown in Table 2. The growth of alpha activity during the first 20 days

Table 1. Thorium alpha activity in Pacific Ocean water.

Sample	Th^{234} tracer recovered (%)	Th alpha activity (disintegration/hr liter)
	Surface	
Fraction S-1	10	9 (±2)
Fraction S-2	40	9.6 (±0.5)
	Deep ocean	
Fraction D-1	28	0.3 ±0.3
Fraction D-2	45	0.4 ±0.4

Table 2. Alpha activity in thorium isolated from surface water, sample S-2.

Elapsed time after chemical processing (day)	Total alpha activity in sample (disintegration/hr liter)	
	Observed	Calcd.
0	9.6 (±0.5)	
3	13.5 (±1.0)	13.5
4	15.2 (±0.9)	14.6
7	16.6 (±0.6)	16.7
18	18.5 (±0.6)	19.0
21	19.1 (±0.6)	
35	18.7 (±0.6)	18.3
73	16.7 (±0.6)	16.0
161	14.8 (±0.6)	14.3
221	13.7 (±0.5)	13.7
363	12.8 (±0.5)	

indicates the presence of Th^{227} or Th^{228}, or both.

The fact that the activity at 73 days is higher than the initial activity shows that Th^{228} is present. If we represent the initial activities (in disintegrations per hour per liter of water) of the individual nuclides by $A_{Th^{232}}$, $A_{Th^{230}}$, and so forth, we may write for the total initial alpha activity

$$9.6 = A_{Th^{232}} + A_{Th^{230}} + A_{Th^{228}} + A_{Th^{227}}.$$

At 363 days over 99.99 percent of Th^{227} and daughters will have decayed. Allowing for Th^{228} and daughter activity growth and decay during the 363-day interval (*3*) and assuming 100 percent Rn^{220} retention, we write for the total alpha activity at 363 days

$$12.8 = A_{Th^{232}} + A_{Th^{230}} + 3.5\, A_{Th^{228}}.$$

At 21 days the activity of Th^{228} and its daughters will be 4.84 times the initial Th^{228} activity (*3*). The activity of Th^{227} and its daughters will be 2.31 $A_{Th^{227}}$. Hence we write for the activity at 21 days

$$19.1 = A_{Th^{232}} + A_{Th^{230}} + 4.8\, A_{Th^{228}} + 2.3\, A_{Th^{227}}.$$

Solving the above equations, we find:

$A_{Th^{227}} = 1.6$ disintegration/hr liter
$A_{Th^{228}} = 1.9$ disintegration/hr liter
$A_{Th^{232}} + A_{Th^{230}} = 6.1$ disintegration/hr liter

Calculated total alpha activities at various times for a sample which initially had this composition are shown in Table 2 alongside the observed activities in the S-2 surface sample. The calculated and observed values are in good agreement.

Taking the uranium content (*4*) of ocean water as 3.0×10^{-6} g/liter ($A_{U^{238}} = 130$ disintegration/hr liter), we see that in both samples the Th^{230} content is far below the amount required for secular equilibrium with the U^{238} present. In the surface water we find 5 percent or less of the equilibrium quantity of Th^{230}; in the deep sample less than 1 percent of the equilibrium amount.

The equilibrium Th^{227} activity corresponding to a uranium content of 3.0×10^{-6} g/liter is 6 disintegration/hr liter. A comparison with the experimental values listed above shows that the Th^{227} concentration in both the deep water and surface water samples is also below its equilibrium concentration with respect to U^{235}, indicating that not only Th^{230} but apparently also Pa^{231} or Ac^{227}, or both, are precipitated with the sediments.

Using the value 1.3×10^{-13} g/liter for the radium (*5*) content of deep ocean water, we calculate $A_{Ra^{226}} = 17$ disintegration/hr liter. Hence, for the deep sample,

$$A_{Th^{230}}/A_{Ra^{226}} < 0.05$$

The radium content of the water is far in excess of the amount which can be supported by the Th^{230} which is present. Koczy, Picciotto, Poulaert, and Wilgain (*2*) report a similar situation in their Skagerak and Gullmerfjord samples. They suggest that the excess radium may arise from redissolution of radium originating from Th^{230} in the sediments (*6*).

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Relationship between Rate of Photosynthesis and Growth of Juvenile Red Salmon

Bare Lake, a 120-acre unstratified lake on Kodiak Island, Alaska, was fertilized annually during the period 1950–56 with inorganic nitrate and phosphate fertilizers. The total amount of fertilizer added each year was calculated to increase the concentration of phosphate phosphorus and nitrate nitrogen by approximately 0.05 and 0.25 mg/liter, respectively.

The purpose of fertilization was to determine whether this process will bring about an increase in the food supply of red salmon (*Oncorhynchus nerka*) during their lake residence, and thereby increase their growth and survival rate prior to their migration to sea, which may occur during the beginning of their second, third, or fourth year of age. Studies have demonstrated that fertilization during the years 1950–53 increased the rate of photosynthesis of the phytoplankton and increased phytoplankton production (*1*).

Phytoplankton are utilized by a variety of benthic insect larvae and zooplankton upon which the fish have been observed to feed. That these organisms have increased in production is strongly suggested by the fact that growth of young red salmon has increased since 1950 (Fig. 1*a*). The seaward-migrating red salmon, generally referred to as smolts, received no benefit from this fertilization in 1950, for they migrated prior to the July application and the juveniles during their first growing season probably received very little benefit by 27 August, the date their growth was calculated.

To obtain information about the size of red salmon smolts prior to 1950, measurements of scale radii were taken of smolt scales for the years 1950–53 and of the fresh-water zone scales from adult salmon that returned to the lake from the smolt migrations of 1947 through 1953. A significant correlation was found between the scale radii and fork length of smolts, and it was found that the fresh-water zone scale radii of adult red salmon were equal to or greater than scale radii measurements from samples of the smolts producing the adults. Since the scale radii of adult red salmon returning from the smolt migrations of 1947–49 were slightly smaller than those returning from the smolt migration of 1950, this is good evidence that the smolts of those years were no larger than those of 1950.

Thus, it appears that fertilization has brought about an increment in fish growth that has to date been rather progressive over the years. It is important to note that during the period no increase in growth of red salmon occurred

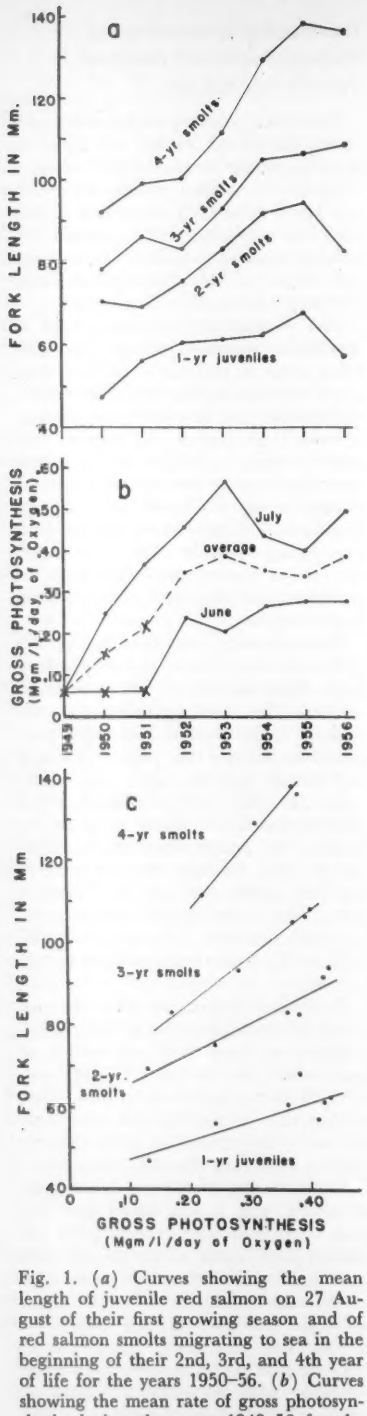

Fig. 1. (*a*) Curves showing the mean length of juvenile red salmon on 27 August of their first growing season and of red salmon smolts migrating to sea in the beginning of their 2nd, 3rd, and 4th year of life for the years 1950–56. (*b*) Curves showing the mean rate of gross photosynthesis during the years 1949–56 for the 40-day periods following the June and July fertilization. Also presented is a curve of the average of the two periods. Points on the curves marked by x's denote the values are estimated or partly estimated. (*c*) Scatter diagrams showing the relation between gross photosynthesis and fork length for each age group of fish. Regression lines are drawn by inspection.

in nearby unfertilized Karluk Lake. Because of the long life cycle of red salmon, data are not yet sufficient to demonstrate whether fertilization has increased the fresh-water survival.

The rate of growth of fish is very sensitive to influence by the food supply. Although plankton and bottom fauna have been sampled regularly, the time-consuming censuses have not yet been completed. However, data are available on the primary productivity as measured by the rate of photosynthesis of the phytoplankton (Fig. 1*b*). These measurements were made by the method originally described by Gaarder and Gran (*2*).

No actual determinations of rate of photosynthesis were made in 1949, but a few determinations were made prior to the July fertilizations of 1950 and 1951, years when the lake was not fertilized in June. On the basis of measurements made during those periods (*1*), it is believed the mean rate of oxygen production would not have exceeded 0.12 mg/liter per day and may well have been about 0.06; the latter figure is plotted as the rate during 1949 and during June of 1950 and 1951. Following the 1951 season the same amount of fertilizer was used as before, but it was applied during two periods, June and mid-July.

A cursory comparison of the curves of seasonal rate of photosynthesis and size of fish reveals a certain correspondence between them (Fig. 1*a* and *b*). To show the relation more clearly, diagrams were made (Fig. 1*c*). It was thought that three periods in time would be of importance in affecting the population size and growth of the new crop of insect larvae hatching in early summer and which would be fed upon by the young juvenile red salmon that had hatched earlier that spring. The period following July fertilization of the year prior was considered important to the survival of the brood stock of insect larvae which was to produce the new generation to be utilized by the fish. Periods following both the June and July fertilizations would influence the growth and survival of the newly hatched larvae. Thus, in Fig. 1*c* the length the first-year juvenile salmon attained each year is plotted against the mean rate of photosynthesis after the June and July fertilizations of that year and the period following the July fertilization of the preceding year. All three periods were weighted equally in establishing the mean. In a somewhat similar manner, smolt size was plotted against the mean rate of photosynthesis over those periods mostly responsible for the development of insect larvae upon which the fish feed during their lake residence.

It might be supposed, since so many steps exist between the original synthesis of food materials by the phytoplankton and growth of fish, and since fish are affected by so many environmental factors in addition to food supply, that a significant correlation would not exist. Nevertheless, the growth of smolts showed a very close relation with the rate of photosynthesis (Fig. 1*c*). The relationship with juveniles at the end of their first growing season is weaker. However, the figures indicate a much closer relation between fish growth and primary photosynthetic productivity than might have been expected a priori (*3*).

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18 March 1958

Role of Cyanoacetic Acid in Production of Lathyrism in Rats by β-Aminopropionitrile

β-Aminopropionitrile (BAPN) is the toxic factor in *Lathyrus odoratus* meal which produces lathyrism in young rats (*1, 2*). The mechanism by which BAPN exerts such profound effects on mesodermal tissue is not known. Metabolic studies have been performed with BAPN in order to gain some knowledge concerning its toxicity. During these investigations, an acidic metabolite of BAPN was discovered in phenol extracts of rat urine. This metabolite has been isolated from the urinary phenols and crystallized (*3*). The chemical structure of the crystalline derivative has proved to be cyanoacetic acid (*4*). Following an injection of C^{14} cyano-labeled BAPN into rats, 80 to 90 percent of the radioactive material is excreted within 20 hours. Approximately 40 percent of the activity is in unchanged BAPN, and 25 to 30 percent can be recovered in cyanoacetic

Table 1. Changes observed in rats following the feeding of aliphatic amines or nitriles.

Assay	Chemical ingested	No. of rats	Wt. gain (g)	Gross alterations
1	None	3 (0)	2.9	None
2	$HOOCCH_2C{\equiv}N$	4 (0)	2.8	None
3	$NH_2COCH_2C{\equiv}N$	4 (0)	2.8	None
4	None	6 (0)	2.6	None
5	$CH_3CH_2NH_2$	6 (0)	2.6	None
6	$HOCH_2CH_2NH_2$	6 (0)	2.5	None
7	$CH_3CH_2C{\equiv}N$	6 (0)	2.5	None
8	$NH_2CH_2CH_2C{\equiv}N$	6 (4)*	1.8	†

* Rats died during period of feeding.
† Gross alterations: Femur, fibrous proliferation, 6; Vertebra, kyphoscoliosis, 2; Aorta, ruptured, 3.

acid. The oxidation of BAPN to cyanoacetic acid in the rat is therefore considerable. Before the role of cyanoacetic acid in the development of lathyrism can be appraised, however, it is necessary to establish whether this compound exerts any influence on mesodermal tissue.

Young (41 to 45 g) female Sprague-Dawley rats were used. Rats in assays 1, 2, and 3 were allowed to eat rat pellets (*5*) ad libitum. Test rats received 200 mg of cyanoacetic acid or cyanoacetamide per 100 ml of drinking water each day for 7 weeks. In other assays the rats were fed a 0.3-percent concentration of the following: ethylamine, ethanolamine, propionitrile and BAPN fumarate (*6*) in a semisynthetic diet (*7*) for 7 weeks. In each assay the rats were housed in an open-bottom mesh cage. Autopsies were performed, and the organs were examined for gross changes. The alterations observed in these assays are shown in Table 1.

Neither cyanoacetic acid nor cyanoacetamide in concentrations at which BAPN produces lathyrism showed any evidence of toxicity (assays 2 and 3). Minor alterations of chemical structure in BAPN as represented by the organic amine or nitrile fed in assays 5, 6, and 7 also resulted in loss of toxicity. The incidence of skeletal deformities and aortic rupture in rats fed BAPN (assay 8) was comparable to that of previous observations (*7*).

Present studies show that cyanoacetic acid, cyanoacetamide, and propionitrile resemble other organic nitriles which do not exert any influence on mesodermal tissue (*2*). The fact that cyanoacetic acid does not affect mesodermal tissue when fed suggests that oxidation of the amine in BAPN to a carboxyl is a mechanism of detoxication. Ethylamine and ethanolamine also failed to produce lathyrism. Skeletal deformities observed in lathyrism, therefore, are not due solely to an excess of an aliphatic amine in the diet. Amines of the general type $R \cdot CH_2NH_2$ are oxidized by amine oxidase to aldehydes (*8*) and may eventually be converted to acids (*9*). Blaschko has suggested that amine oxidase might function in detoxication of some toxic amines (*10*). β-Aminopropionitrile is a toxic amine which fosters in some manner the development of skeletal deformities, herniation, and aortic rupture in young rats. The recovery of cyanoacetic acid from urine of rats adminstered BAPN suggests that amine oxidase is involved in detoxication of BAPN. Since cyanoacetic acid does not produce skeletal deformities, tissue changes observed in lathyrism are probably caused by either BAPN or cyanoacetaldehyde (*11*).

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14 February 1958

Dynamics of Release of Histamine from Tissue Mast Cell

It is generally acknowledged that the tissue mast cell contains histamine and heparin (*1*). This report (*2*) is concerned with a series of cytological changes that we have observed in living mast cells treated with histamine liberators. Our findings extend those previously reported (*3*) in living mast cells treated similarly. Microscopic observations and cinephotomicrographic recordings were made of the mast cells of the transilluminated mesentery of the intact, anesthetized Sprague-Dawley rat. Bright-field illumination and magnifications of 400 to 900 were employed. The experiments consisted of supplanting the oxygenated Tyrode's solution normally bathing the preparation with Tyrode's solution containing one of the following test substances: Compound 48/80 (*4*), 1:100,000; stilbamidine, 1:80 to 1:8000; protamine sulfate, 1:5000 to 1:100,000; or toluidine blue, 1:5000 to 1:200,000. All of these compounds bring about the release of histamine from the mast cell (*5*).

Prior to treatment the mast cells of the mesentery are round or spindle-shaped and densely packed with dark granules. Shortly after the introduction of any of the above test solutions there occurs a marked change in the refractile properties of the granules: they suddenly lose their dark appearance and become almost invisible. First one granule and then another reacts until all have become involved and the cell is barely discernible. Correlated with these events is a gradual swelling of the cell to about 1⅓ times its original diameter.

After toluidine-blue treatment, the nucleus of the mast cell takes on a blue color when about 50 to 80 percent of the granules have lost their dark appearance. When most or all of the granules are scarcely visible, metachromatic staining of the faded granules begins. At first a few granules stain purple, then more and more, until apparently all are so stained. As the staining of the granules proceeds, the mast cell shrinks toward its normal size. At this stage, those living cells stained with toluidine blue resemble closely mast cells in mesenteries fixed in alcohol and then stained with toluidine blue (*6*).

I consider the present findings to be indicative of significant chemical changes in the mast cell. The changes in the refractile properties of the granules are interpreted to be a manifestation of the freeing of some material from binding either within or on the surface of the granule where it is osmotically inactive. Once free, the material is osmotically active; water enters the cell and swelling results. It seems likely that the material liberated is histamine which is freed from its known binding with heparin (*7*).

The cytological changes noted here are common to a variety of treatments that cause release of histamine from the mast cell, and the time course of the changes is the same as that for histamine release resulting from such treatments (*5*). I suggest that histamine is freed from its binding with heparin because the histamine liberators have a stronger affinity for heparin than does histamine. The sequence of changes in the experiments with toluidine blue is consistent with such an interpretation; toluidine blue does not stain the granules until binding sites are made available on molecules of heparin. When the molecules of toluidine blue bind heparin, they become osmotically inactive and the cell loses water and becomes smaller in size. The movement of histamine out of the cell at this time also contributes to the loss of water and shrinking of the cell. According to the present interpretation, heparin is not lost from the mast cell treated with histamine liberators.

DOUGLAS E. SMITH

Division of Biological and Medical Research, Argonne National Laboratory, Lemont, Illinois

References and Notes

1. G. P. Fulton, F. I. Maynard, J. F. Riley, G. B. West, *Physiol. Revs.* **37**, 221 (1957).
2. This work was performed under the auspices of the U.S. Atomic Energy Commission.
3. H. U. Zollinger, *Experientia* **6**, 384 (1950); I. Mota, W. T. Beraldo, L. C. U. Junqueira, *Proc. Soc. Exptl. Biol. Med.* **83**, 455 (1953).
4. Compound 48/80, a polymer of N-methylhomoanisylamine and formaldehyde, was supplied through the courtesy of Dr. Edwin J. de Beer of the Wellcome Research Laboratories.
5. D. E. Smith, *Abstr. of Communications, 20th Intern. Physiol. Congr., Brussels* (1956), p. 835; D. E. Smith, *Am. J. Physiol.* **193**, 573 (1958).
6. A description of the details of the late events following treatment with the other test substances is in preparation.
7. E. Wehrle and R. Amann, *Klin. Wochschr.* **23**, 624 (1956).

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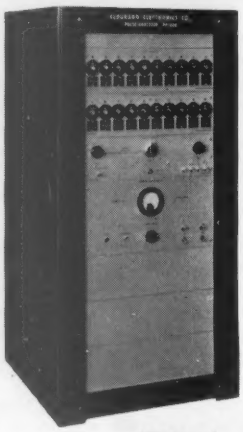

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Letters

Temperature of Earth Satellites

In the issue of 11 April [*Science* **127**, 811 (1958)], Raymond H. Wilson, Jr., used a short-cut method for computing the temperature of an earth satellite. He concluded that the thermal radiation from the earth is not important, because earth and satellite are at nearly the same temperature. I question this conclusion.

It is true that the *net* exchange between the two will be small. But the calculation requires equating the *total* radiation absorbed by the satellite with that radiated by it *in all directions*. The solar radiation intercepted by the earth is practically equal to the sum of the solar radiation reflected and the thermal radiation from the earth. The average figure for the sum of the intensities of the two latter radiations, at a height of 10 percent of the earth's radius, will be $1/(4 \times 1.1^2) = 0.206$ of the intensity of radiation received directly from the sun.

A satellite in an equatorial orbit would probably average somewhat more than this proportion, and one in a polar orbit somewhat less. On the basis of this figure, a gray satellite, never in the earth's shadow, would reach a temperature 5 percent greater than that calculated on the basis of solar radiation alone, and one in the earth's shadow 20 percent of the time would reach the same temperature as that calculated for exposure to the sun's radiation alone 100 percent of the time.

WILLIAM D. ROSS
Wilmington, Delaware

William D. Ross's extension of my "short-cut" treatment of solar radiation to a discussion of satellite heating due to the earth seems to be a fruitful contribution. By treating the sum of the mean terrestrial radiation and reflection as constant (since the earth's mean temperature is constant), he has not only indicated the correct effect of heat radiated by the earth but has also furnished an independent check on my result for the added temperature due to its reflection of sunlight. Of the total of 15°C which he finds to be added by the earth, I had already included 4°C for reflection alone, corresponding to the 30 percent albedo assumed for the earth. The new estimate of temperature for a continuously sunlit satellite having equal absorptivity and emissivity would thus be 311°K.

However, for such low-temperature infrared radiation as the terrestrial, the absorptivity/emissivity ratio would be almost exactly unity for all types of satellite surfaces, and its flux would be practically independent of the fraction of time the satellite is sunlit. These char-

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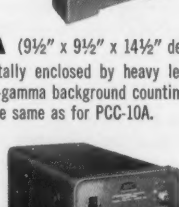

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acteristics of its effectiveness are thus fundamentally different from those of direct and reflected sunlight. The temperature 311°K is the highest mean to be expected for satellites at the particular 400-mile mean height assumed by Ross. For heights above this, the terrestrial contribution falls off rapidly, as his treatment makes clear.

Another possible variation which should be borne in mind is that a satellite temperature decrease of 20°C would result from assuming the 400° lower effective solar temperature 5760°K which is implied by direct measurements of the solar "constant." Also, it is quite certain that the 2-percent eccentricity of the earth's orbit would cause annual variations up to 1 percent, or 3° above and below the mean. An approximate general formula, incorporating all these considerations, would be:

$$T = T_s(D_s/4)^{1/6} \times [1 + (1 + 2\varepsilon/\alpha F)/48(1 + h/3960)^2] \times (\alpha F/\varepsilon)^{1/4}$$

In this, T_s and D_s are, respectively, the effective absolute temperature and radian diameter of the sun, α/ε is the absorptivity/emissivity ratio of the satellite, F its sunlit time fraction, and h the mean height of its orbit in miles.

RAYMOND H. WILSON, JR.
Project Vanguard, U.S. Naval Research Laboratory, Washington, D.C.

Absorption Spectra of Hill Reaction Oxidants

In considering the correlation of absorption spectra with oxidant potential and with antioxidant ability, as in the report of R. J. Marcus, J. L. Hatchett, and K. M. Sancier [*Science* **127**, 647 (1958)], it is of interest to note that F. J. Stubbs and C. N. Hinshelwood [*J. Chem. Soc. Suppl. Issue* **1**, 571 (1949)] showed that with arylamines the activation energy of acylation was partially contributed by the ring substituents. These contributions were additive. Stubbs and Hinshelwood also showed that other properties, such as basicity and absorption spectra, correlated similarly with these contributions.

It is reasonable to assume that, for the phenols and phenoxy radicals related to the quinones of the report of Marcus, Hatchett, and Sancier, the ring substituents control the energy of dissociation of the weakest hydrogen. Perhaps both the redox potential and the absorption spectra of these quinones and their reaction intermediates can then be considered as functions of this dissociation energy.

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Meetings

Forthcoming Events

August

24–28. American Inst. of Biological Sciences, annual, Bloomington, Ind. (H. T. Cox, AIBS, 2000 P St., NW, Washington 6.)

The following 25 meetings are being held in conjunction with the AIBS meetings at Bloomington, Ind.

American Bryological Soc., annual. (Mrs. V. S. Bryan, Botany Dept., Duke Univ., Durham, N.C.)

American Fern Soc., annual. (Miss M. E. Faust, 501 University Pl., Syracuse 10, N.Y.)

American Microscopical Soc., annual. (D. G. Frey, Dept. of Zoology, Indiana Univ., Bloomington.)

American Phytopathological Soc., 50th anniversary. (W. B. Hewitt, Dept. of Plant Pathology, Univ. of California, Davis.)

American Soc. for Horticultural Science, annual. (R. E. Marshall, Dept. of Horticulture, Michigan State Univ., East Lansing.)

American Soc. of Ichthyologists and Herpetologists, annual. (R. Conant, Philadelphia Zoological Garden, 34th and Girard Ave., Philadelphia 4, Pa.)

American Soc. of Limnology and Oceanography. (B. H. Ketchum, Woods Hole Oceanographic Inst., Woods Hole, Mass.)

American Soc. of Naturalists. (B. Wallace, Long Island Biological Assoc., Cold Spring Harbor, N.Y.)

American Soc. of Parasitologists, annual. (P. E. Thompson, Research Div., Parke, Davis & Co., Detroit 32, Mich.)

American Soc. of Plant Physiologists, annual. (G. R. Noggle, Dept. of Botany, Univ. of Florida, Gainesville.)

American Soc. of Plant Taxonomists. (R. F. Thorne, Botany Dept., State Univ. of Iowa, Iowa City.)

American Soc. of Zoologists. (S. Crowell, Dept. of Zoology, Indiana Univ., Bloomington.)

Biometric Soc., ENAR. (T. W. Horner, General Mills, Inc., 400 Second Ave., S., Minneapolis 1, Minn.)

Botanical Soc. of America, annual. (H. C. Bold, Dept. of Botany, Univ. of Texas, Austin 12.)

Ecological Soc. of America. (J. E. Cantlon, Dept. of Botany and Plant Pathology, Michigan State Univ., East Lansing.)

Mycological Soc. of America, annual. (E. S. Beneke, Dept. of Botany and Plant Pathology, Michigan State Univ., East Lansing.)

National Assoc. of Biology Teachers. (P. Fordyce, Broad Ripple High School, Indianapolis, Ind.)

Nature Conservancy. (G. B. Fell, 4200 22 St., NE, Washington 18.)

Phycological Soc. of America, annual. (W. A. Daily, Dept. of Botany, Butler Univ., Indianapolis 7, Ind.)

Potato Assoc. of America, annual. (R. V. Akeley, Crops Research Div., USDA, Plant Industry Station, Beltsville, Md.)

Society for Industrial Microbiology, annual. (C. L. Porter, Dept. of Biological Sciences, Purdue Univ., West Lafayette, Ind.)

Society of Protozoologists, annual. (N. D. Levine, College of Veterinary Medicine, Univ. of Illinois, Urbana.)

Society for the Study of Development and Growth. (R. O. Erickson, Dept. of Botany, Univ. of Pennsylvania, Philadelphia 4.)

Society of Systematic Zoology. (R. E. Blackwelder, Box 500, Victor, N.Y.)

Tomato Genetics Cooperative. (E. C. Stevenson, Horticulture Dept., Purdue Univ., West Lafayette, Ind.)

24–29. Atmospheric Diffusion and Air Pollution, intern. symp., Oxford, England. (F. N. Frenkiel, Applied Physics Laboratory, Johns Hopkins Univ., Silver Spring, Md.)

24–29. Mental Health, world federation, 11th annual, Vienna, Austria. (Miss E. M. Thornton, World Federation for Mental Health, 19 Manchester St., London, W.1, England.)

24–30. Astronautical Cong., 9th intern., Amsterdam, Netherlands. (A. G. Haley, International Astronautical Federation, 1735 DeSales St., Washington.)

24–30. Prehistoric and Protohistoric Science, 5th intern. cong., Hamburg, Germany. (Büro des Internationalen Kongresses für Vor- und Frühgeschichte, c/o Fremdenverkehrs- und Kongresszentrale,

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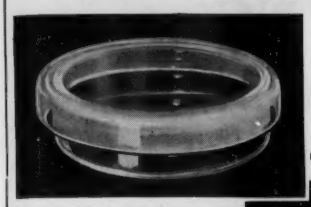

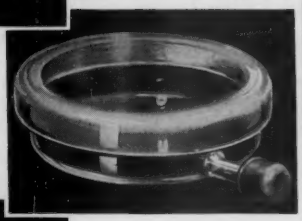

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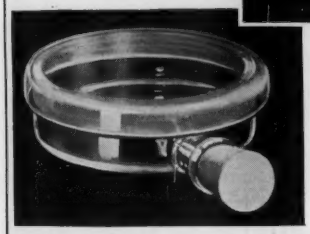

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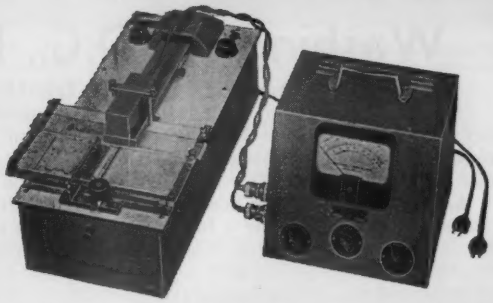

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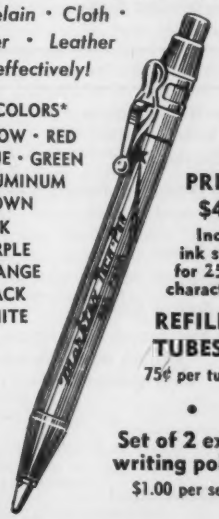

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25–28. Institute of Mathematical Statistics, annual, Cambridge, Mass. (G. E. Nicholson, Jr., Dept. of Statistics, Univ. of North Carolina, Chapel Hill.)

25–28. Mathematical Assoc. of America, 39th summer, Cambridge, Mass. (H. M. Gehman, Univ. of Buffalo, Buffalo 14, N.Y.)

25–29. Infrared Spectroscopy Inst., annual, Nashville, Tenn. (J. R. Lawson, Fisk Univ., Nashville 8.)

25–29. Electronic Properties of Metals at Low Temperatures, IUPAP colloquium, Geneva, N.Y. (M. D. Fiske, General Electric Co., P.O. Box 1088, Schenectady, N.Y.)

25–30. American Mathematical Soc., 63rd summer, Cambridge, Mass. (AMS, 190 Hope St., Providence 6, R.I.)

27–29. American Sociological Soc., annual, Seattle, Wash. (Miss M. W. Riley, ASS, New York Univ., Washington Sq., New York 3.)

27–29. Diseases in Nature Communicable to Man, 13th annual intern. Northwest conf., Hamilton, Montana. (W. L. Jellison, National Microbiological Inst., Rocky Mountain Laboratory, USPHS, Hamilton.)

27–3. British Assoc. for the Advancement of Science, Glasgow, Scotland. (BAAS, Burlington House, London, W.1, England.)

28–2. Biometric Soc., ENAR, Ottawa, Ont., Canada. (T. W. Horner, General Mills, Inc., 400 Second Ave., S., Minneapolis 1, Minn.)

28–3. Combustion Symp., 7th intern., London and Oxford, England. (Combustion Inst., 936A Union Trust Bldg., Pittsburgh 19, Pa.)

31–6. Housing and Town Planning, 24th cong., Liége, Belgium. (International Federation for Housing and Town Planning, Parkhotel, Molenstraat 53, The Hague, Netherlands.)

31–8. Corpuscular Photography Colloquium, 2nd intern. (by invitation), Montreal, Canada. (P. Demers, Institut de Physique, Universite de Montreal, P.Q.)

September

1–6. Biochemistry, 4th intern. cong., Vienna, Austria. (O. Hoffmann-Ostenhof, 1, Chemisches Institut der Universität, Währingerstrasse 42, Vienna IX.)

1–7. Psychotherapy, intern. cong., Barcelona, Spain. (M. de la Cruz, Clinica Psiquiatrica Uniersitaria, Facultad de Medicina, Barcelona.)

1–9. Analogy Computation, 2nd intern., Strasbourg, France. (F. H. Raymond, 138 Boulevard de Verdun, Courbevoie, Seine, France.)

1–13. Peaceful Uses of Atomic Energy, 2nd intern. conf., Geneva, Switzerland. (L. D. P. King, Atomic Energy Commission, Washington 25.)

2–4. Allergy, 4th European cong., London, England. (British Assoc. of Allergists, St. Mary's Hospital, London, W.2.)

2–4. Vertebrate Phylogeny Symp., Soc. of Vertebrate Paleontology and Soc. for the Study of Evolution, Ann Arbor, Mich. (J. T. Gregory, Peabody Museum, Yale Univ., New Haven, Conn.)

(See issue of 18 July for comprehensive list)

Cary / instrument abstracts

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The conventional dithizone colorimetric procedures can be used to estimate the lead concentration to an accuracy of approximately 10 parts per billion. A refinement of this procedure, employing the Cary Model 14 Recording Spectrophotometer, is used to more precisely determine the concentration.

The color intensity of the lead dithizone solution is measured at 5100 Angstroms for the unknown sample and for two standard solutions whose concentrations are respectively a little more and a little less than the estimated concentration of the unknown. By interpolating, the analyst can then determine the lead concentration of the unknown to an accuracy of one part per billion or better.

In this procedure, the high photometric accuracy of the Model 14 is of primary importance in reliably recording the minute differences in absorbance values between the sample and standards. This high photometric accuracy is one of several performance features provided in each Cary Recording Spectrophotometer to a degree not found in any other similar instruments. Perhaps these advantages can lead to new breakthroughs in your analytical techniques. Complete information on both Cary Spectrophotometers, Model 11 and Model 14, is contained in a bulletin which is available upon request. Ask for Data File E10-78.

BRIEF SPECIFICATIONS OF CARY SPECTROPHOTOMETERS

	MODEL 11	MODEL 14
RANGE	2100Å to 8000Å	1860Å to 2.6 microns.
STRAY LIGHT	Less than 0.0001% over most of working range.	Less than 0.0001% between 2100Å and 1.8 microns; less than 0.1% at 1860Å and 2.6 microns.
SCANNING SPEEDS	From 1.0Å/sec. to 125Å/sec.	From 0.5Å/sec to 500Å/sec.
RESOLUTION	Better than 1.0Å throughout range.	Better than 1.0Å U.V.-visible region and 3.0Å near-infrared.
WAVELENGTH ACCURACY	Better than 5.0Å U.V. region and 10.0Å visible region.	Better than 4.0Å throughout range.
REPRODUCIBILITY	Better than 0.5Å U.V. region and 3.0Å visible region.	0.5Å throughout range.
PHOTOMETRIC REPRODUCIBILITY	0.004 in absorbance.	0.002 in absorbance.

Equipment

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Science *does not assume responsibility for the accuracy of the information. A coupon for use in making inquiries concerning the items listed appears on page 218.*

■ VACUUM GAGE of thermocouple type uses a nonlinear scale to cover the range from 0 to 1000 μ-Hg with a least indication of 5 μ. Exposure to atmosphere will not cause damage. The instrument is obtainable with either one or two sensing tubes and in models suitable for operation from either a 115- or 230-v supply. (Consolidated Electrodynamics, Dept. 172)

■ FURNACE for growing single crystals of semiconductor materials can produce crystals up to 12 in. long. Temperatures up to 1550°C, controllable through a 15-kw saturable-core reactor to within ±1°C are provided. Crucible capacity is more than 9 in^3. A graphite double-helix resistive heating element is used. Crystal-pulling speed is continuously variable from 0 to 9.75 in./hr. Interchangeable heads permit operation under either inert gas or vacuum. (N.R.C. Equipment Corporation, Dept. 177)

■ DISSOLVED-OXYGEN ANALYZER automatically and continuously measures dissolved oxygen in high-purity water. Traces as low as 1 part in 10^9 are detected. Ranges include 0 to 25, 0 to 50, 0 to 250 parts per billion and a tenfold extended range with the upper portion of the scale compressed for leak tracing during boiler start up. Signal output is 0 to 5 mv and is suitable for input to standard potentiometric recorders. Time for 90-percent response is approximately 2 min. Accuracy is ±5 percent of reading. Calibration and zero are stable to within ±1 part in 10^9 for periods of weeks over an ambient temperature range 50° to 110°F. Accessories include a multiple-sample system for up to four streams, a calibrator, and temperature and pressure regulators. (Beckman Instruments, Dept. 182)

■ LABORATORY OVEN covers the range from room temperature to 200°C. Inside dimensions are 12 by 10 by 10 in. Air circulation is controlled by a thermostat. Shelves are adjustable in ½-in. increments. Models are available for operation at 110 or 220 v. (Grieve-Hendry Co., Inc., Dept. 189)

■ D-C AMPLIFIER is transistorized and equipped with a self-contained solid-state power supply. Band pass is from d-c to 50,000 cy/sec. Noise is less than 8 μv. Drift rates are expected to be less than 2 μv/wk. Gain is 0 to 1000. (Electro Instruments Inc., Dept. 199)

■ REFRIGERATED BATH is available in two standard sizes of 2.72- and 6.50-gal capacity. The refrigeration equipment includes a hermetically sealed compressor. Range of temperatures is 0° to 100°C with control accuracy of ±0.25°C. Two frequently used temperatures may be set for selection at any time, undisturbed by intervening use at other temperatures. Time required to reduce temperature from 32°C to 0°C is 120 min for the smaller model, 240 min for the larger. (Blue M Electric Co., Dept. 185)

■ DIAPHRAGM PUMP for low-volume pumping of oil-free air is designed for continuous operation. Vacuum up to 22 in.-Hg and pressure up to 40 $lb/in.^2$ are produced. Maximum displacement is 1 ft^3/min under free-flow conditions. Weight is 16.7 lb. Models are available for 115-v or 220-v operation. (Air-Shields Inc., Dept. 183)

■ CENTRIFUGE for testing electronic components produces accelerations as high as 50,000 *g*. Acceleration is read directly on a meter which functions through a selector switch, set to the specimen-mounting radius, and an electronic counter that indicates speed of rotation. Slip-ring assemblies are available as optional equipment. Set speeds are reached in less than 1 min. After running for a preset time, the machine automatically brakes to a stop. (American Machine and Foundry Co., Dept. 190)

■ MICROWAVE OSCILLATORS cover the range from 1 to 18 kMcy/sec in five swept ranges. Radio-frequency output is 10 mw minimum except in the 12 to 18 kMcy/sec range, in which the output is 1 mw. Frequency variation is linear with time, and sweep rate is adjustable from 10 cy/ to 0.01 cy/sec. (Alfred Electronics, Dept. 186)

■ AUTOMATIC TESTER can select any two of 240 points and measure resistance between them in the range of 1 ohm to 9.99 megohm. The tester may be programed to select 1, 5, 10, or 20 percent nominal tolerance or to pass any value above or below a selected median resistance. Testing rate is 60 to 100 tests per minute. (Lavoie Laboratories, Inc., Dept. 192)

■ COMPUTER LINKAGE SYSTEMS permit analog and digital computers to be interconnected. The system is of modular design. Data from the digital computer are converted to analog voltages in the range ±100 v and presented on command to the analog computer; similarly, analog voltages are converted to digital code form, 11 binary digits, including sign. Analog-to-digital conversion time is 22.5 μsec; digital to analog conversion time is 5 μsec. (Epsco, Inc., Dept. 200)

■ DECIMAL KEYBOARD for integration into electrical and electronic systems contains ten decimal and three optional-assignment keys. Single-pole, double-throw switches operated by the keys will handle 3 amp at 125 v a-c or 2 amp at 30 v d-c. The unit is available with or without housing and cable. (ElectroData Division, Burroughs Corp., Dept. 191)

■ MASSIVE TURNTABLE for mounting complete navigation system platforms weighing up to 500 lb achieves rotational and alignment accuracies of ±2 sec of arc. The table is tiltable ±90 deg. It is driven by synchronous motor at various multiples of the earth's rate of rotation. (Sterling Precision Corp., Dept. 193)

■ WAVE ANALYZER produces in permanent-record form an analysis of any repetitive complex waveform. Plots are either amplitude versus frequency, power versus frequency, or amplitude of a specific frequency versus time. Frequency range is 3 to 10,000 cy/sec. Frequency accuracy is ±1 cy/sec from 3 to 50 cy/sec, and ±2 percent from 50 to 10,000 cy/sec. Amplitude accuracy is ±5 percent of reading or ±0.2 percent of full scale, whichever is larger. (Minneapolis-Honeywell Regulator Co., Dept. 194)

■ ISOLATED POWER SUPPLY designed for transistor circuits measures ⅞ by 1⅛ by 2½ in. and weighs less than 3 oz. Output voltages range from 4 to 26 v, currents from 9 ma at 4 v to 1.5 ma at 26 v. Output is Zener-diode regulated. Shunt capacitance to ground is 20 pf; leakage resistance to ground exceeds 10,000 megohm. (Elcor Inc., Dept. 195)

■ MOTION-PICTURE CAMERA is capable of speeds up to 400 frames per second. Two 16-mm models provide 100- and 200-ft capacities, respectively. Film is stopped completely during exposure and locked in position by a register pin. Operation is on 28 v d-c or 115 v a-c. (D. B. Milliken Company, Dept. 196)

■ SCRATCH INVESTIGATION MICROSCOPE, manufactured by Hilger & Watts in England, can be used in the field to measure scratches up to 0.025 in. deep on flat or curved surfaces. Accuracy is 0.001 in. A camera attachment permits making photographic records on 35-mm film. (Engis Equipment Company, Dept. 198)

■ TEMPERATURE-SENSING RESISTOR is a silicon solid-state device with a positive temperature coefficient of resistance of 0.7 percent/°C. Two configurations are available, an axial-lead molded form and a welded-case form. Resistance ratings range from 100 to 1000 ohm at 25°C. (Texas Instruments Inc., Dept. 184)

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National Bureau of Standards

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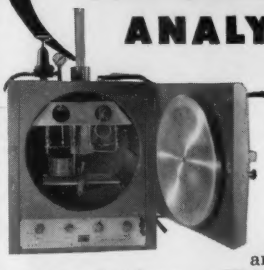

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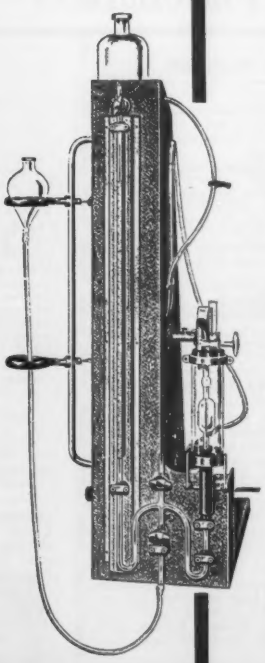

The apparatus consists essentially of the following:

(a) Shaking burette in water jacket
(b) Graduated manometer
(c) Connecting tube assembly
(d) Mercury reservoir
(e) Water reservoir

The Van Slyke Gas Apparatus is used mainly for the determination of carbon dioxide in blood, carbon monoxide in blood and plasma, and similar determinations where an amount of gas evolved from a fluid has to be measured.

Phipps and Bird adheres strictly to the principles set forth by Dr. Van Slyke when he pioneered this apparatus for manometric gas analysis.

The measuring manometer is mounted over a narrow opening in the front panel, behind which there is a strip of light-conducting plastic. The assembly is illuminated from the rear, the illumination being confined to the manometer and is uniform throughout. The reaction chamber is illuminated through an opening on the side of the manometer housing and from the same light source.

The manometer is protected by a clear plastic shield.

The apparatus is supplied complete with glass parts as illustrated.

Cat. No. 21-432

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Manufacturers & Distributors of Scientific Equipment
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1958 Model *Heavy Duty*

GYROTORY® SHAKER

A dependable, maintenance-free Rotary Action Shaker for general laboratory work when thorough shaking and vigorous agitation for sustained periods of time are required.

140 to 400 oscillations per minute (alternate speed range—50 to 150)

1" eccentric rotary motion

two platform sizes—18" x 30"—24" x 36"

interchangeable platforms available

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Platforms hold Erlenmeyer flasks, test tubes or other glassware.

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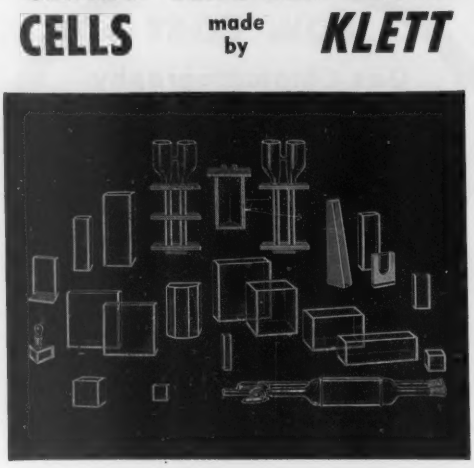

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POSITIONS WANTED

Bacteriologist, M.S. Excellent background and experience. Desires to relocate. Supervise hospital bacteriology laboratory. Prefer Florida. Box 180, SCIENCE. X

Biochemist, Ph.D., 1957, 28, married; year's postdoctoral terminating, teaching experience. Desires academic or industrial position. Hormonal control lipid metabolism, steroid metabolism. Broad biological background. Box 173, SCIENCE. 7/25

Entomologist, Ph.D., minor in physiology: 1 year teaching in experiment station. Desires research academic position. Box 11, SCIENCE.

POSITIONS WANTED

Embryologist; Ph.D.; prefers teaching and research; presently in charge biochemical laboratory, research institution; in early 30's. Medical Bureau, Burneice Larson, Director, 900 North Michigan Avenue, Chicago. X

Medical Writer-Translator (German, Romance languages); experience also abstracting, editing. B.A.; AMWA. Box 178, SCIENCE. X

Microbiologist, M.A.; 5 years of research experience in immunology, chemotherapy. Available for suitable position in September. Box 177, SCIENCE. 8/8

Ph.D., Microbiology-Biochemistry; 5 years of industrial and academic experience, fermentation processes, chromatography, bioassay, manometric, and radioisotope techniques. Seeks industrial position. Box 179, SCIENCE. 8/1, 8

POSITIONS OPEN

(a) Bacteriologists; M.S. or better for outstanding opportunities, new cancer research hospital; superb equipment, facilities; to $12,500; eastern college city. **(b) Pharmacologist;** M.S., Ph.D., for independent research in inflammaion, analgesia, sympatholytic drugs; to $10,000; important midwestern cosmetics firm. **(c) Bacteriologist;** modern, well-equipped laboratory, 350-bed general hospital; some research opportunity; to $6000 or better; city 60,000 near mideastern university city. **(d) Chemist;** Ph.D., equivalent experience to head chemistry laboratory, assist pathologist in general laboratory administration, share in varied teaching programs; research opportunity; large university affiliated hospital; to $9000; East. Woodward Medical Bureau, Ann Woodward, Director, 185 North Wabash, Chicago. X

Experienced Cytotechnologist. Papanicolaou Research Laboratory, 1300 York Avenue, New York 21, New York. uc

POSITIONS OPEN

(a) Chemist, Biochemist or Physical Chemist; supervisory position, research department, major industrial company established 1894; college town, East. **(b) Ph.D.** in nutrition or chemistry; full-time research position; home economics department of state college, South; most of work has been related to nutritive value of foods as affected by environmental and other factors. **(c) Biochemist,** Ph.D. or exceptionally highly qualified M.S.; duties include research and supervising staff of nine; 600-bed general hospital; plans completed for new 900-bed hospital; Midwest; $8000–$10,000. **(d) Scientist** with pharmaceutical or allied academic training and several years' industrial experience in pharmaceutical research development; Ph.D. helpful but not required; should be qualified to project coordination, large group of technical people; university city, East; $8000–$12000. S7-4 Medical Bureau, Burneice Larson, Director, 900 North Michigan Avenue, Chicago. X

Chemist, male, with B.S. degree in organic chemistry or biochemistry, to participate in analytical biochemical research in mental illness in hospital near Philadelphia. Write stating age, education, experience, and salary desired to P.O. Box 8507, Philadelphia 1, Pa. 7/25; 8/1, 8, 15

Chemist, male or female. Assistant, Atomic Energy Commission research project; training in biochemistry. Personnel Department, Albert Einstein Medical Center, York and Tabor Roads, Philadelphia 41, Pa. X

SCIENCE TEACHERS, LIBRARIANS, ADMINISTRATORS urgently needed for positions in many states and foreign lands. Monthly non-fee placement journal since 1952 gives complete job data, salaries. Members' qualifications and vacancies listed free. 1 issue, $1.00. Yearly (12 issues) membership, $5.00. CRUSADE, SCI., Box 99, Station G, Brooklyn 22, N.Y. ew

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Medical Microbiologist, Ph.D.; experience in laboratory organization, administration, development; widely traveled, language. Wishes permanent position private company or hospital, New York City area; will travel. Box 176, SCIENCE. X

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Pharmacologist, Ph.D., to head small, growing department, expanding pharmaceutical manufacturer. Require ambitious, imaginative, productive scientist, under 35, seeking career opportunity in industrial pharmacology. Please send complete résumé, including publications and references. Box 170, SCIENCE. 7/25

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25 July 1958

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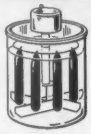

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This is the first monograph specifically devoted to data from the experimental production and prevention of tooth decay in laboratory animals. Twenty distinguished investigators, representing dentistry and allied basic sciences, report and review their researches. Unusually well organized, the book represents a coherent sequence, and is skillfully summarized by the editor in a final chapter, followed by very detailed subject and author indexes.

AAAS

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"OUR PREVIOUS AD

created a great deal of interest, for which we are grateful. Additional advertising in your magazine is contemplated."

25 July 1958

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Use this easy self-mailer to obtain further information about items or literature from the Equipment section as well as from advertised products.

EQUIPMENT

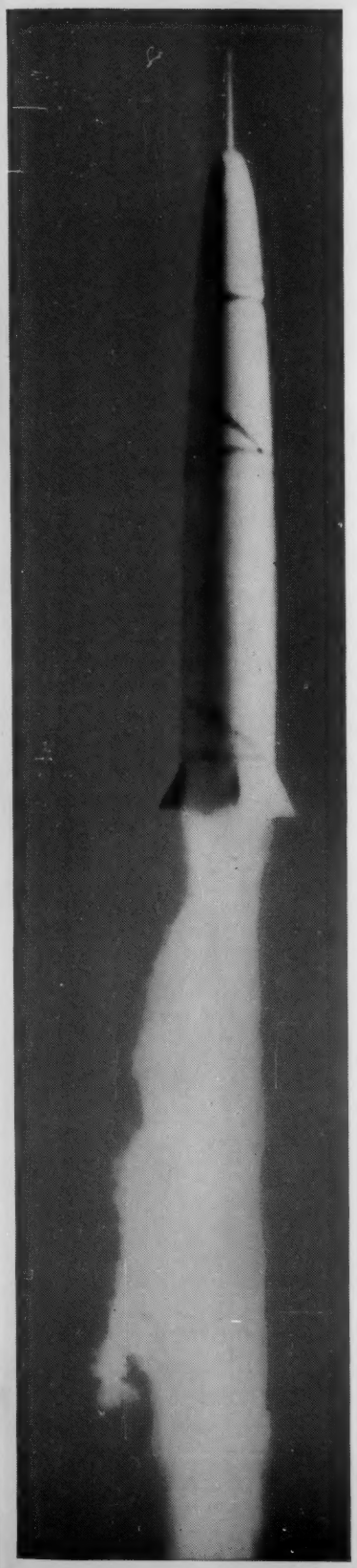
Circle below desired number corresponding to:

172	177	182	183	184	185	186	189	190
191	192	193	194	195	196	198	199	200

ADVERTISERS IN THIS ISSUE

In list below, check page number of advertiser from whom you would like more information. If more than one item appears in ad, letters (A, B, C) are used to indicate particular items available in order of appearance in advertisement. Where more than one ad appears on page, **"U" indicates upper ad, "L" lower ad, "I" inside ad, "M" middle ad,** and **"O" outside ad.** Advertisements in Personnel Placement and Market Place are not keyed. A multiplicity of items is indicated by *. Readers are requested to specify on this coupon the particular item in which they are interested; otherwise, the request cannot be processed.

☐ 170*	☐ 171	☐ 172, A	☐ 172, B	☐ 174
☐ 208, O	☐ 208, I-A	☐ 208, I-B	☐ 208, I-C	☐ 209
☐ 210	☐ 211, UI	☐ 211, UO	☐ 211, L*	☐ 213
☐ 215, UI	☐ 215, UO*	☐ 215, L	☐ 216, UO	☐ 216, UI
☐ 219	☐ 220			



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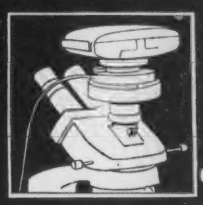
THE MOST VERSATILE PHASE MICROSCOPE EVER MADE

Phasestar's building block concept of design, exclusive with AO Spencer, offers you a choice of more than 50 different models.



Low cost 35 mm. camera for crisp, inexpensive photomicrography. Coupled system lets you shoot what you see.

Choice of 3 interchangeable full 360° rotatable bodies, monocular, binocular, and trinocular, inclined for comfort. Special inclined monocular and vertical monocular bodies for photomicrography available.

No other Phase Microscope can offer you such a wide choice of interchangeable parts and accessories.

You can adapt the Phasestar to your exact needs with various combinations of readily interchangeable bodies, stages, bases and optics. You have more than 50 possible combinations or models to choose from.

Your Phasestar will be "just right" for you in convenience, comfort and the economy of real quality.

Focus stage and specimen to the objectives with low-positioned coarse and fine adjustments. Choose from 3 interchangeable stages, graduated or ungraduated mechanical stages or the new Micro-Glide circular stage.

Your choice of interchangeable horseshoe base with double-plano mirror in fork mount or built-in base illuminator.

The large fork type condenser mount allows you to quickly interchange a wide selection of Phase condensers . . . turret type, single unit or special long focus.

Quick-change quadruple revolving nosepiece lets you interchange complete sets of phase, achromatic and apochromatic objectives quickly and precisely.

American Optical Company

SPENCER

INSTRUMENT DIVISION, BUFFALO 13, NEW YORK

● Dept. G-4

Please send me the new Phasestar brochure

Name.....

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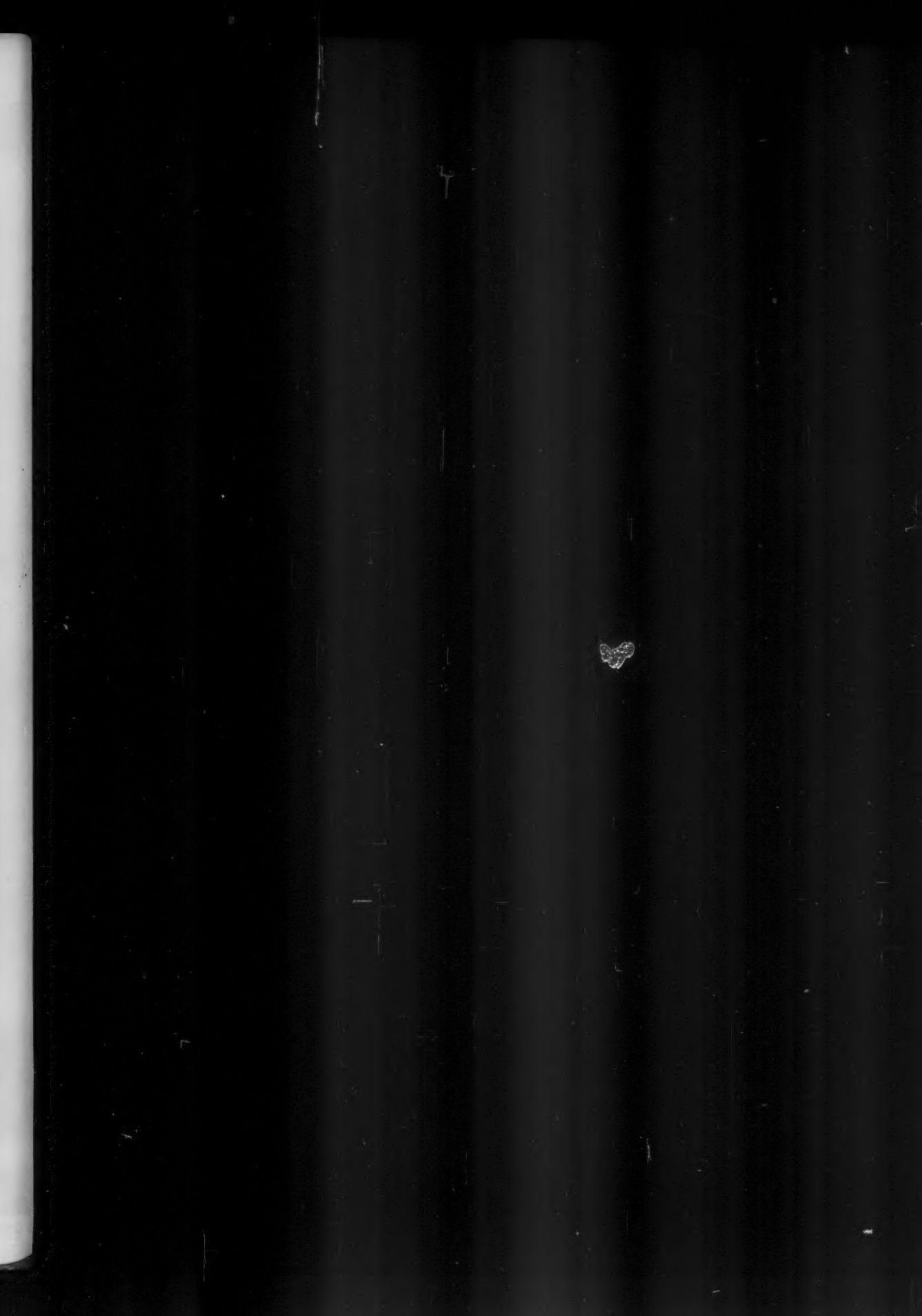
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